To
Dan McKenzie Esq:–

With the author's compliments

and kindest regards.

a

ENGLISH
MEN OF SCIENCE

EDITED BY

J. REYNOLDS GREEN, D.Sc.

JOHN DALTON

ENGLISH MEN OF SCIENCE

Edited by J. REYNOLDS GREEN, D.Sc.

HERBERT SPENCER. By J. ARTHUR THOMSON

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Yours truly
John Dalton

JOHN DALTON

BY

J. P. MILLINGTON

M.A., B.Sc.

FORMERLY SCHOLAR OF CHRIST'S COLLEGE
CAMBRIDGE

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PREFACE

In the following account of the life and work of Dalton I have availed myself of the material contained in the following memoirs:—

Memoirs of the Life and Scientific Researches of John Dalton, by W. C. Henry.

Memoir of John Dalton, by Robert Angus Smith.

The Worthies of Cumberland. John Dalton, by Henry Lonsdale.

John Dalton and the Rise of Modern Chemistry, by Sir H. E. Roscoe.

In addition to these I am indebted for much valuable information to *A New View of Dalton's Atomic Theory*, by Sir H. E. Roscoe and Dr. Arthur Harden. From the same book, by the kind permission of Messrs. Macmillan & Co., the accompanying portrait of Dalton is taken.

My best thanks for much kindly help are due to my friend, Mr J. M. F. Drummond of Caius College, Cambridge.

J. P. MILLINGTON.

London: *August* 1906.

CONTENTS

ATOMIC SYMBOLS

BY

JOHN DALTON, D.C.L., F.R.S, Etc., Etc.

EXPLANATORY OF A

LECTURE

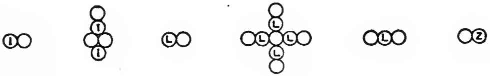
GIVEN BY HIM TO THE MEMBERS OF THE MANCHESTER MECHANICS' INSTITUTION

October 19th, 1835

ELEMENTS

Hydrogen . . .	⊙	*Azote* . . .	⦶
Carbon . . .	●	*Sulphur* . . .	⊕
Zinc . . .	Ⓩ	*Tin* . . .	Ⓣ
Oxygen . . .	○	*Chlorine* . . .	(Ch)
Phosphorus . .		*Lead* . . .	Ⓛ
Iron . . .	Ⓘ	*Copper* . . .	Ⓒ

OXIDES



SULPHURETS

COMPOUNDS

Binary

Water . . . 

Nitrous Gas . . 

Carbonic Oxide . 

Sulphuretted Hydrogen . . . 

Phosphuretted Hydrogen . . 

Olefiant Gas . . 

Cyanogen . . 

Ternary

Deutoxide of Hydrogen . . . 

Sulphurous Acid . 

Acetic Acid . . 

Nitrous Oxide . 

Carbonic Acid . 

Phosphoric Acid . 

Nitrous Vapour . 

Carburetted Hydrogen . . . 

Prussic Acid . .  ?

Bicarburetted Hydrogen . . 

Tan . . . 

Quaternary

Sulphuric Acid . 

Binolefiant Gas . 

Pyroxylic Spirit . 

Quinquenary

Ammonia . . 

Nitrous Acid . 

Prussic Acid . .  ?

Sexenary

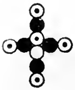
Alcohol . . 

Pyroacetic Spirit 

Septenary

Nitric Acid . . 

Decenary

Ether . . 

INTRODUCTION

At the present time when the attentions of chemists and physicists are being drawn, more forcibly than ever, perhaps, to the fundamental questions of the nature of matter, when ions, atoms and corpuscles are words in daily use by layman and scientist alike, it is interesting to turn aside and consider the life and work of one who, by his genius and perseverance prepared the way for those who came after him.

The subject of this memoir laid the foundation stones; others have helped to build the great structure of modern chemistry. Just as one brilliant hypothesis concerning the nature of the benzene molecule put forward by August Kekulé in 1865 led to the development of the vast subject of the chemistry of the cyclic compounds, which in recent years has been applied by many workers to technical uses of vast importance, so too the enunciation by Dalton of the atomic hypothesis was the signal for the commencement of those labours which have made the science of chemistry the mighty subject that it is.

Dalton's work is truly epoch-making, for it clearly marks the passage from the older empiricism to the newer rational processes of thought which serve to guide the chemists of to-day. He did not invent the idea of

an atomic or discontinuous structure of matter; he elaborated the speculations of the Greek philosophers, and put forward the view that all compounds are composed of atoms which combine according to fixed rules which he himself in great part ascertained. Even now, a century after the world first heard of Dalton's hypothesis and the laws of combination, we have but little to add to our knowledge of these laws. The hypothesis still serves to explain the countless observations made since its enunciation; the great array of facts which has been marshalled since Dalton's time contains nothing to contradict the laws he laid down, and though during the last few years discoveries concerning radioactive phenomena, and the researches of Professor J. J. Thomson would seem to indicate the existence of mysteries more profound than any that entered into Dalton's philosophy, though we apparently stand on the verge of some new and great generalization, yet the atomic hypothesis, with those modifications which, by reason of greater knowledge have been introduced, still forms the basis for chemical reasoning.

The story of Dalton's life is practically the story of his work. Unlike Lavoisier he took no prominent part in stirring questions of the day. As far as we know he was a man of deep religious feeling, yet he never appeared to the world as a controversialist in theological matters as did his great predecessor Priestley. In private life he was upright and straightforward, loyal to his friends, and above all things devoted to duty, which for him meant work.

In the following pages an attempt is made to give an

account of the work to which he gave his life. The results he obtained were

> "The slow
> Uncertain fruit of an enhancing toil."

In his case the toil was amply rewarded, for he lived to see his views universally adopted and made an integral and necessary part of the subject of chemistry. As an experimenter, Dalton was far behind Davy or Berzelius. No new analytical method, no great discovery of an experimental nature can be set to his credit. His work was of a different order; it consisted in making use of facts recorded by himself and others to explain phenomena. All the experiments he did make seem to have been devised with the object of testing some theory.

For fifty-seven years he patiently recorded meteorological observations, and from these he deduced theories concerning climatic phenomena. Indeed, as a result of his meteorological work he came to study the atmosphere, at first as such, and later as a gas or "elastic fluid," and it was from these studies that he derived the idea of atomic structure.

Beyond his scientific work, there is little to say of him, for he played but a small part in social life. The child of simple Quaker people living in a Cumberland village, he remained to the end unaffected, shy, even awkward. With none of the culture of Davy, with few of the graces of others more worldly-wise than he, Dalton remained undisturbed by the outer world, content to live his quiet life and carry out his patient

investigations. He carried out Descartes' *morale par provision* by seeking pleasure rather in limiting his desires than in attempting to satisfy them, and by making the search after truth the business of his life. The key-note of his character is perseverance.

"If I have succeeded better than many who surround me, it has been chiefly, nay, I may say, almost solely from unwearied assiduity. It is not so much from any superior genius that one man possesses over another, but more from attention to study, and perseverance in the objects before them, that some men rise to greater eminence than others." In these few words he sums up the essential features of his life and character.

His position as a scientist and his claims to remembrance are plain. As an experimental chemist, he left no mark in chemistry, but as a law-giver he ranks among the greatest. Without his atomic hypothesis, the progress of science would have been greatly retarded, because of it chemistry has advanced with mighty strides to the place which it occupies to-day.

John Dalton

CHAPTER I

JOHN DALTON'S EARLY LIFE AND EDUCATION

John Dalton was born at Eaglesfield in Cumberland on the 6th of September 1766. His ancestors were of lowly birth and earned their living either in the practice of husbandry, or by labour that was not highly skilled. The history of his ancestry can be traced no further than the middle of the seventeenth century, when Thomas Fearon, born at Eaglesfield in 1658, married Mary Gill of the same village. The marriage took place in 1688, and in 1690 a daughter Abigail was born, who married, some twenty-two years later, Jonathan Dalton, the grandfather of John Dalton.

Of the children of this marriage only two need be mentioned, the eldest Jonathan, and the youngest Joseph. The former became a farmer, inheriting all the estate of his father, a shrewd, capable man, who added to his wife's dowry certain property obtained by purchase. There was no issue to this marriage of Jonathan Dalton the younger with Mary Thompson of Gilcrux, so that on the death of the widow (who survived him four years) the Dalton estate fell to the brother Joseph.

Joseph Dalton was a weaver of cheap woollen goods

for country use. His trade was a common one in those days, for the north-country people wore but little cloth that was not woven at home or linen not home-spun.

He was generally regarded as rather careless—Lonsdale describes him as "feckless",—and earned only a scanty living at his trade. For once, however, he left the common rut, and went to Caldbeck to court Deborah Greenup whom he eventually married at the Meeting-house in Cockermouth on 10th June 1755. Deborah seems to have been an active, energetic woman, quite different from the easy-going Joseph with whom she threw in her lot.

By this marriage of Joseph Dalton with Deborah Greenup there were six children. Three died early, and the other three, Jonathan, Mary and John, grew to maturity. Jonathan, the eldest, was born on 9th September 1759, Mary on 24th January 1764, but there is no record of the date of birth of John, and it was only years later, when he had become famous, that it was ascertained that he was born about the 6th of September 1766. This uncertainty is due to the fact that the Quakers, to which denomination the Daltons belonged, have no form of baptism, and naturally the name of John Dalton appears in no parish register. The customary registration appears to have been omitted and so we are left without authoritative evidence as to the precise date when the great philosopher was born.

As soon as he was old enough to be useful, John was taught to help his father in his trade of weaving by doing light work, such as preparing the shuttles and holding the spools. His early instruction in mathe-

matics he obtained from his father, but he also attended the Pardshaw Hall School then directed by Mr. John Fletcher, a man of considerable ability and foresight, who taught on more rational lines than was common at that time. Many years later, Dalton referred to these school days and his early education as follows :—

"The writer of this was born at Eaglesfield, near Cockermouth, Cumberland. Attended the village schools there, and in the neighbourhood, till eleven years of age, at which period he had gone through a course of mensuration, surveying, navigation, etc."

At about this time he came under the notice of Mr. Elihu Robinson, a gentleman of property in whose employ was a young man named Alderson. Alderson and Dalton studied together under the direction of Mr. Robinson, and it is related that, some dispute having arisen, Alderson proposed to settle it by means of a small wager of sixpence. Their patron however suggested that the loser should supply his fellow-student with candles for reading during the winter months. The suggestion was adopted and Dalton secured the wager. The story is told as illustrating the difficulty with which education was acquired in those days. Dalton, in common with his fellows was compelled to assist on his father's farm in the summer, and it was only during the long winter evenings that time could be spared to pursue his studies.

At the age of twelve he began teaching in a school in his native village of Eaglesfield, but whether this was the one in which he had received his education or another has not been determined. For a short time he

taught his pupils in an old barn, later in his father's house, and finally in the Friends' Meeting-house. On the outside of the house, or on the front door, Dalton posted a written announcement that he had opened a school for both sexes on reasonable terms. Further he informed the public that "paper, pens and ink," were to be obtained within, so it must be presumed that these articles necessary for education were difficult to buy in the neighbourhood.

One of his pupils, John Robinson, has described him as having a severe struggle to maintain discipline in this school, for many of the learners were as old as he and frequently challenged him to fight in the graveyard outside. The only successful punishment seems to have consisted in locking them in the schoolroom during the dinner hour, though even this plan sometimes failed owing to their escape through the windows which they had broken in revenge.

In 1781, being then fifteen years old, he gave up the school in Eaglesfield and went to Kendal as assistant to his cousin George Bewley. His brother Jonathan had been in the same school for some time, and eventually the two brothers carried on the work. The Daltons were not popular as teachers on account of their uncouth manners and sternness in matters of discipline, but from all accounts John seems to have been the more popular, largely, it is to be feared, on account of his want of vigilance, for his mind was often so occupied with mathematical problems that evil-doers escaped their just punishments. On one occasion, however, corporal punishment was administered. The culprits were held

by John, while Jonathan used the whip so sharply that blood was drawn, and the assistance of a surgeon was rendered necessary.

It was at this time that he first came in contact with John Gough, a man blind from infancy but of considerable scientific attainments, and it was largely owing to his influence that Dalton formed those orderly habits of mind which show so strikingly in his works.

In the preface to his *Meteorological Observations and Essays*, 1793, he refers to Gough in the following terms:—"To one person, more particularly, I am peculiarly indebted, not only in this respect but in many others; indeed if there be anything new and of importance to science, contained in this work, it is owing, in great part, to my having had the advantage of his instruction and example in philosophical investigation." In the second edition of the same work published in 1834 he refers openly to Gough as having been his intimate friend and adviser for eight years.

In the following letter written from Kendal in 1788, Dalton describes the character and attainments of Gough. He writes:—

"John Gough is the son of a wealthy tradesman in this town (Kendal). Unfortunately he lost his sight by the small-pox when about two years old, since which he has been quite blind, and may now be about thirty. He is, perhaps, one of the most astonishing instances that ever appeared of what genius, united with perseverance and every other subsidiary aid, can accomplish when deprived of what we usually reckon the most valuable sense. He is a perfect master of the Latin,

Greek, and French tongues: the two former of which I knew nothing of six years ago, when I first came here from my native place near Cockermouth, but, under his tuition, have since acquired a good knowledge of them. He understands well all the different branches of mathematics, and it is wonderful what difficult and abstruse problems he will solve in his own head. There is no branch of natural philosophy but what he is well acquainted with; he knows by the touch, taste, and smell almost every plant within twenty miles of this place; he can reason with astonishing perspicuity on the construction of the eye, the nature of light and colours, and of optic glasses; he is a good proficient in astronomy, chemistry, medicine, etc., etc. His father, being very able, has furnished him with every necessary help, and would have sent him to the University had he been inclined. He has the advantage of all the books he has a mind for, which others read to him; he employs one of his brothers or sisters to write for him, or else myself, especially in mathematical attempts; he has never studied the art of writing much, or I doubt not he would succeed. He and I have been for a long time very intimate. As our pursuits are common — viz., mathematical and philosophical—we find it very agreeable frequently to communicate our sentiments to each other and to converse on those topics."

For many years Dalton was in the habit of submitting his work to Gough. Dalton's paper on the constitution of mixed gases was the occasion for severe criticism by his former teacher. Gough states that he

was "compelled to make an open attack on his friend Mr. Dalton, and his new convert Mr. Henry." Pupil and master denounced each other's arguments as unsound, and the controversy became somewhat bitter in tone. Dalton implied that Gough was ignorant of chemistry and used illustrations which, if forcible, erred perhaps in the matter of good taste. To this Gough replied that his pupil had "amused the superficial reader rather than convinced the reasoner." He further accuses him of having treated the matter with acrimony and ridicule. In answer to this charge Dalton promises to avoid as much as possible these defects and with regard to a dynamical objection to his theory says that "having studied the principles of Dynamics as well as those of many other mathematical and physical sciences, under the tuition of Mr. Gough, he feels under strong obligations to him; but these, he will readily grant, do not bind him to subscribe to his opinions when he cannot perceive them to be well-founded."

Like so many controversies of this nature, the one in question was largely due to misunderstanding, and doubtless Gough had no difficulty in forgetting the incident when his pupil became famous as a philosopher in the front rank of scientists.

Apparently it was Gough's example that led him to make and record his first scientific observations. These took the form of a diary which he called *Observations upon the Weather*. This was begun 24th March 1787, and was continued until the day before his death. At first this journal consisted merely of remarks upon the state of the weather, but subsequently included the readings of

thermometer, barometer and hygroscope. Dr. Henry had in his possession two volumes of this journal which contained observations for the years 1787-1793 in Kendal, and 1793-1803 in Manchester.[1] Dalton describes his instruments and methods as follows:—

"The preceding and following observations on the temperature of the weather were made on instruments of my own construction. The barometer is graduated into sixteenths of an inch. The thermometer is mercurial, with Fahrenheit's scale, exposed to the open air, but free from the sun. The hygroscope is about six yards of whip-cord, suspended from a nail with a small weight to stretch it; its scale, tenths of inches, beginning from no certain point,—the less the number the shorter the string and the greater the moisture." His instruments were scarcely those of precision, for in a minute description of a barometer which he made for Mr. Crosthwaite, there is no mention of his having made any attempt to get rid of air and moisture from the tube. This defect he recognised soon afterwards, for he writes to Crosthwaite, "moisture is above all things else to be avoided as it depresses the mercury far more than a particle of air does."

The following extracts from a letter to Miss Hudson will serve to show how enthusiastic he was over meteorological phenomena:—

"KENDAL, 8 *mo.*, 4th, 1788.

"RESPECTED FRIEND,—The study of Nature having been with me a predominant inclination, it is not unlikely

[1] These two volumes are now in the archives of the Literary and Philosophical Society of Manchester.

that I should be ready to prompt others to the same. I have been tempted to think that thou would take a pleasure in remarking the quantity of rain which falls with you each day, if thou knew with what facility the same is effected. I have observed here that people who are entirely ignorant of the matter suppose it a work of great labour and difficulty, and which can only be done by those they call great scholars. This, however, is a great mistake. A very little knowledge of mensuration is sufficient for the theory of it, and nothing but plain addition is wanted in the practice."

Then follows a description of the apparatus employed, and of the manner in which the results are calculated and expressed. He concludes:—

"By this time I apprehend the difficulty generally supposed to attend this matter is removed. I should be glad if thou, or any other in your neighbourhood, on whose accuracy one might rely, would find it agreeable and convenient to notice this matter; but, however, I do not mean to request it, but only to show the easiness with which it's done. Ignorance, no doubt, will look upon this as a trifling and childish amusement, but few of this nature are such in a philosophical sense. If to be able to predict the state of the weather, with tolerable precision, by which great advantages might accrue to the husbandman, to the mariner, and to mankind in general, be at all an object worthy of pursuit, that person who has in any manner contributed to

attain it cannot be said to have lived or to have laboured in vain.—I am respectfully, thy friend,

JOHN DALTON.

"To SARAH HUDSON,
Eaglesfield."

In another letter, written to his cousin and former teacher, Elihu Robinson, he describes his method of making thermometers. The letter is of sufficient interest to justify quotation, as it gives us some idea of his early experimental work. It runs as follows:—

"KENDAL, 8 *mo.*, 23rd, 1788.

"DEAR COUSIN,—Herewith thou wilt receive, I hope safely, two thermometers with somewhat longer scales than the former; please to take thy choice of the three, to let John Fletcher have the next choice, and to reserve the other till my brother comes.

"You will probably chuse by the length of the scales; but those with the least bulbs will soonest come to the temperature of the surrounding medium. However, the largest, I apprehend, will rise or fall to within a degree of the proper place in half an hour in the air. Thou may try whether that thou hast already is with these two or not, by dipping the bulbs into a bason of water for five minutes.

"Possibly the manner of making them may not be unentertaining. A small receptacle being fixed on the end of the tube, a quantity of mercury is poured into it, part of which runs down the tube so as to half fill the bulb, and then stops, the tube being still filled with mercury, which is unable to fall by reason of the

pressure of the air in the bulb. Then a candle is applied to the bulb, which, rarefying the air contained in it, raises the mercury in the tube quickly to the top, and then it escapes in bubbles through the mercury in the receptacle. This done, it is cooled again, when, the internal air contracting, another portion of mercury falls down into the bulb; and this operation is repeated till all the air is expelled. Then the mercury is heated above boiling water, and the end of the tube melted and closed at the same time, when, the mercury subsiding, there is left a vacuum; this is done chiefly to keep the moisture, dust, etc., out of the tube. The whole is then put into boiling water, when the barometer stands at 30 inches, and the boiling point thereby determined; afterwards (if circumstances admit) the freezing point is found by putting it into a mixture of water and pounded ice, or water and snow, which, when melting before the fire, keep at an invariable point (32°) till the whole is melted. If this cannot be done, as in summer, it may be set by another thermometer, and the scale adapted accordingly. N.B.—As the freezing points of these two were not found on account of the season, it will not be amiss to try whether they are accurate, when a convenient season comes.

"The principles on which they act need little explanation; as mercury, like most other bodies, is subjected to be contracted by cold, and expanded by heat; and as the capacity of the bulb remains always filled, the total variation of the mercury in bulk, it is evident, will be manifested in the tube.

"The range of the thermometer is little in these parts compared with the more northern. At Petersburgh the summer heat is equal to ours, but in winter severe cold predominates; the thermometer is frequently found 40 or 60 below nothing; and in Siberia it has been observed even 100 or 120 below nothing.

"On the contrary, in the burning sands of Africa it reaches 100 or 140 above nothing. Is not the internal principle of heat in man and other animals a wonderful phenomenon, that can sustain these two extremes without sensible variation? Remark.—Réamur's scale, used by the French and others, counts from 0 at the freezing point to 80 at the boiling point; consequently 2¼ degrees Fahrenheit are equal to 1 of Réamur.

"ABSTRACT OF MY JOURNAL FOR THE PRESENT YEAR.

Thermometer without.				Rain—Inches and Decimals.	Wet Days.	Auroræ Boreales.
	Mean.	Highest.	Lowest.			
1 mo.	39·0	47	20	5·6160	20	6
2 mo.	38·3	47	28	3·3064	23	2
3 mo.	36·8	50	18	2·8183	16	4
4 mo.	46·3	69	32	2·9047	16	11
5 mo.	53·0	80	38	1·1872	10	7
6 mo.	57·3	80	45	2·3137	7	2
7 mo.	56·8	68	47	7·0323	28	1

"THUNDERSTORMS.

"5 mo., 19. 2 P.M., distant, W.

"5 mo., 26. 7 P.M., frequent loud peals, very near.

"7 mo., 3. 6 P.M., frequents peals, some very near.

"8 mo. 16. 7½ P.M., distant about 8 miles, S.E., but loud and tremendous; about 20 or 30 flashes were observed in as many minutes, and the reports of each heard, though the cloud was but just visible above the horizon; the zenith clear. My love to Cousin Ruth, self, and family. JOHN DALTON."

During the progress of his intimacy with Gough, he continued his teaching work, now with an increased responsibility, for in 1785 George Bewley retired from the school at Kendal, and the two brothers Jonathan and John Dalton announced, in the following terms, their intention of continuing the school:—

"Jonathan and John Dalton respectfully inform their Friends and the Public in General, that they intend to continue the school taught by George Bewley, where Youth will be carefully instructed, in English, Latin, Greek and French; also Writing, Arithmetic, Merchants' Accounts, and the Mathematics. The school to be opened on the 28th of March 1785.

N.B.—Youth boarded in the Masters' house on reasonable terms."

The capital required to conduct this school was raised by loans from George Bewley and other friends and relations. Their first year's earnings amounted to £107 and out of this sum they managed to pay off all debts. It appears that they further increased their income by performing various duties which could only be done by those who had had the advantage of an education better than that usually given to the simple village folk. Amongst others, these duties included "searching

registers," "making wills," "drawing conditions," and even "collecting rents."

In addition to this school work, this indefatigable worker announced his intention of giving a course of twelve lectures on Natural Philosophy. The subscription to the whole course was half-a-guinea, or one shilling per lecture if taken singly. He remarks in his announcement that "subscribers to the whole course will have the liberty of requiring further explanations of subjects that may not be sufficiently discussed or clearly perceived when under immediate consideration, and also of proposing doubts, objections, etc.; all which will be illustrated and obviated at suitable times, to be mentioned at the commencement."

"A Syllabus of the Lectures.

"*First and Second*—Mechanics.—Introduction—Rules of Philosophising on Matter and its properties with the different opinions of the most famous philosophers on this head—The laws of motion—Mechanic powers—Vibration of pendulums.

"*Third, Fourth and Fifth*—Optics.—Preliminary discourse—Of the nature and properties of light—Of simple vision—Doctrine of colours—Of reflected vision—Of mirrors and images reflected from them—Of refracted vision with the nature of lenses and images exhibited thereby—Of burning glasses—Description of the eye—Manner of vision—Of long and short-sighted eyes—Of spectacles, telescopes, and microscopes—Of the rainbow.

"*Sixth and Seventh*—Pneumatics.—Of the atmosphere

—The elasticity of the air—Description of the air-pump —The spring and weight of the air proved by a great variety of experiments on the air pump—Of respiration —Of sound—Of winds—Of the blueness of the sky—Of twilight.

"*Eighth, Ninth and Tenth*—ASTRONOMY.—Introduction —Of the solar system—Of the figures, magnitudes, distances, motion, etc., of the sun, planets, and comets —Of the progressive motion of light—Of the fixed stars and their phenomena—Of the lunar planets—Of eclipses, tides, etc.

"*Eleventh and Twelfth*—USE OF THE GLOBES.—Figure of the earth—Description of the globes—Various problems performed thereon, amongst which are, an explanation of the phenomena of the harvest moon and the variations of the seasons—Conclusion.

'Ex rerum causis supremam noscere causam.'"

It appears probable that this course of lectures was not particularly well attended, for we find that a few years later he repeated it, with an additional lecture on Fire, the fees for this course being only half the amount of those for the previous one—a poor enough remuneration for such an extensive field of work.

During this period he derived a certain amount of recreation and no doubt some small pecuniary advantage from contributions to the *Gentleman's Diary* and the *Ladies' Diary*. These contributions took the form of answers to questions on various subjects—mathematical and philosophical. One or two may be quoted as an example of the incongruity of such work with his daily occupations.

"Query I. Whether to a generous mind, is the conferring or receiving an obligation the greater pleasure?

"*Answered by Mr. John Dalton of Kendal.*

"The pleasure arising from conferring an obligation, especially if it be effected without much inconvenience, is pure, and must be a grateful sensation to a generous mind; but that arising from receiving an obligation is often mixed with the unpleasing reflection of inability to remunerate the benefactor. It is pretty clear, therefore, that the pleasure of conferring an obligation must exceed that of receiving one.

"Query II. Is it possible for a person of sensibility and virtue, who has once felt the passion of love in the fullest extent that the human heart is capable of receiving it (being by death, or some other circumstance, for ever deprived of the object of its wishes), ever to feel an equal passion for any other object?

"*Answered by Mr. John Dalton of Kendal.*

"It will be generally allowed that in sustaining the disappointments incident to life, true fortitude would guard us from the extremes of insuperable melancholy and stoic insensibility, both being incompatible with your own happiness and the good of mankind. If, therefore, the passion of love have not acquired too great an ascendency over the reason, we may, I think, conclude that true magnanimity may support the shock without eventually feeling the mental powers and affections enervated and destroyed by it, and con-

sequently that the query may be answered in the affirmative. However, if this passion be too strong, when compared with the other faculties of the mind, it may be feared that the shock will enfeeble it, so as to render the exercise of its functions in future much more limited than before."

It may seem strange that one whose mind was generally occupied with the phenomena of nature should condescend to answer vapid questions in a somewhat vapid style; possibly it was done with a view to self-improvement, or it may be that questions of any kind gave him a chance of exercising the faculty of grappling with problems—a faculty which later developed into a marvellous power of logical reasoning. If this be so, the same characteristic is exemplified in an investigation which he made as to the nature of English surnames. The following extract is from a letter to his friend, William Alderson of Eaglesfield.

"INVESTIGATION OF ENGLISH SURNAMES.

"*First.*—Of those ending in—SON.

"We have a large tribe of these from Christian or first names, such as John, Jack, Harry, Dick, Richard, William, Will, Tom, Robin, Robert, Ben, Allen, etc. —that is, the father being called John, his son was called John's son or Johnson, etc. Also diminutives of these; as Dickin, Wilkin, Tomlin, Jenkin, etc.—son; that is little Dick's son, etc.

"A few, probably bastards from women's names; as Ann, Elly, Matty, Nel, Patty, etc.—son.

"Some from other surnames; as Cook, Smith, Hodge, Dodge, Dod, Dob, Hood, etc.—son.

"*Second.*—Another custom seems to have obtained in the south part of the kingdom, that is using the genitive case of the father's name instead of the word 'son' at the end of it; thus we there meet with Stephens, Roberts, Phillips, Edwards, Harrys or Harris, Jones (that is Joan's or John's), etc., which in the north are more commonly Stephenson, Robertson, etc. From this it may be suspected the Harris families in the north were originally from the south, otherwise they would most likely have been called Harrisons.

"*Third.*—Another source of surnames we have from ancient and trading towns; as York, Chester, Kendal, etc. . . . Besides these a great number from places of less note ending in *ton* (*i.e.*, town), *thwaite* (a place cleared of wood); as Braithwaite, Crosthwaite, Dalton (= dale-town), Norton, Westtown, etc.

"To these may be added a few from the names of nations; as Scot, English, Ireland, French, etc.

"Also a number derived from the situation of their dwellings; as Fell, Gill, How, Hill, etc.

"*Fourth.*—A vast number from trades, etc.; as Smith, Wright, Weaver, Webster, etc. . . . Also from articles, etc., dealt in, as Hay, Stone, Steele, Bell, etc.

"*Fifth.*—From animals; as Fox, Tod (an old word for a fox), Stag, Hinde, Kid, Lamb, Drake, etc.

"*Sixth.*—Some adjectives; as Black, Blake, Dun, White, Brown, etc.

"*Seventh.*—A few ending in *man;* as Bulman, Cow-

man, Bowman, Chapman, etc. Also several ending in *ley*, as Ainsley, Bayley, Brinkley, Priestley, etc.

"*Eighth*.—Compound words of pretty obvious origin; as Brockbank, Sowden, Langmire, Mirehouse, Waterhouse, Salthouse, Crossfield, Swinburn, etc."

Although his time must have been fully occupied with his school work and lectures, he directed his attentions to botany and collected many specimens which he pressed and dried.

Eleven volumes of his *Hortus Siccus* are (or were in 1856) extant, and the first one bears the title—

"*Hortus Siccus seu Plantarum diversarum in Agris Kendal vicinis sponte nascentium Specimina, Opere et Studio Johannis Dalton collecta, et secundum classes et ordines disposita.* 1790."

Nor did botany alone amongst the Natural History Sciences, attract his attention, for he became a collector of the commoner insects, and part of his collection, at anyrate, was to be seen in the Museum at Keswick. With reference to this matter he says in a letter to Crosthwaite, "Some of these may be thought puerile; but nothing that enjoys animal life, or that vegetates, is beneath the dignity of a naturalist to examine." Even in this branch of Natural Science he was not content to make observations only, but devised experiments, for the purpose of determining certain physiological facts. Amongst these experiments must be mentioned one which he conducted upon himself. This consisted in determining the amount of food taken and the amount ejected from the body. The difference between the two, he decided, would give the loss by insensible

perspiration. His results were not published until some forty years later, but the fact that these observations were made is interesting as showing the direction in which his inclinations lay. It appears that he was genuinely interested in vital phenomena, so much so, that he actually went so far as to consult at least two friends on the advisability of taking up one of the learned professions. In a letter to his cousin Elihu Robinson he writes as follows :—

"KENDAL, 4 *mo.*, 8th, 1790.

"DEAR COUSIN,—The occasion of my addressing thee at this time is a projected change of my occupation, which I have been meditating on for some time past, in which thy countenance or disapprobation cannot fail of having due weight.

"I have but one objection to my present business which, however, is a very material one, and a very rational one; that is, the emoluments attending to it are not sufficient to support a small family with the decency and reputation I could wish, should it fall to my lot to have it to do. As to the making of a fortune by it, that's entirely out of the question. I much doubt whether there is one person in the kingdom (amongst friends I mean) who has, after a laborious life, reached independence by it. Indeed, very few people of a middling genius, or capacity for other business will be found willing to undertake it, for the obvious reason assigned above.

"I hope thou wilt not impute the above sentiments to the momentary chagrin of some disappointment, or to

the gloom of a declining school, as neither of these causes exist in any degree; they are the result of mature consideration and unbiassed judgment.

"Thou wilt next expect I should signify what way my inclination has led me, as I may now be presumed capable of judging for myself, after having reviewed the vast varieties of trades, arts, sciences and professions with which the country abounds. Though I doubt not but my inclination would yet adapt itself to any business that promised to be of advantage, yet it seems most natural to turn to such wherein literary or scientific knowledge is requisite, as my pursuits and acquisitions hitherto have been chiefly of this nature. At the head of these stand law and physic. Whether of these professions would be more likely for me to make a livelihood, or whether would require more time and expense to attain, I cannot tell; but, interest being set aside, I should much prefer the latter.

"The great objections are the expense at first, and the uncertainty of getting business afterward; but these, though great, I think, are not insurmountable. To qualify for a physician, three winters' study at Edinburgh will be indispensable; the board for six months may perhaps be had for £10 or £15, and the college fees will be about 12 guineas each season; the two intermediate summers may be employed in some sort of business, which will render the plan as frugal as possible. Now, putting the case at the worst, that I spend most or all of my effects in this scheme, and cannot succeed at the last, I may then return to my present employ, as places are frequently vacant nearly as profitable as this.

"Upon the whole the plan does not appear to me chimerical, and I should be glad to know thy sentiments upon it, at or before the time of the ensuing meeting at Lancaster. I have not yet acquainted friends here with it. Please also to inform us how and where my mother is. Our quarterly meeting is on the 18 and 19 instant.

"Were I disposed to amuse thee a little, I might add some experiments I have lately made to determine the quantity of matter discharged from the body daily by insensible perspiration, etc., which I have made for two weeks successively; and other particulars, as that I have practised as a quack for some time past with good success; but further of these some other opportunity.

"I hope this will find you all well, as it leaves us, and am thy affectionate cousin, JOHN DALTON.

"TO ELIHU ROBINSON,
Eaglesfield, near Cockermouth."

The letter is characteristic of the man—cautious, far-sighted, almost coldly calculating in its methodical statement of his reasons for change. Characteristic too is the reply of his gentle Quaker cousin who wrote, "As I have thought thy talents were well adapted to thy present profession, I cannot say thy proposal of changing it was very welcome to me; believing thou wouldst not only shine, but be really useful in that noble work of teaching youth."

He sought further and possibly more expert opinion from his uncle Thomas Greenup, who was a barrister

living and practising his profession in London, and from him he received no undecided answer in the following uncompromising terms:—

"As to the two professions of law and physic, if thou wishest to be at the head of one of those professions—that is to be at the bar or to be a physician—I think they are both totally out of the reach of a person in thy circumstances. . . . If thou art tired of being a teacher, and wishest to change it for some more lucrative or agreeable employment, and couldst be content, instead of becoming a physician or barrister, to move in the humbler sphere of apothecary or attorney, thou mightest, perhaps, be able, with a little capital and great industry, to establish thyself in one of these."

And so, perhaps as a result of the advice received from these two persons, Dalton decided to continue in his calling as a teacher of youth. Had his decision been made otherwise, the advance of science, in all probability, would have been arrested for years, for it is only reasonable to suppose that, with the arduous duties of a country practitioner, he would have been unable to find time for those researches and speculations upon the results of which the mighty superstructure of modern chemistry has been reared.

CHAPTER II

METEOROLOGICAL INVESTIGATIONS—REMOVAL FROM KENDAL TO MANCHESTER

After deciding to abandon all ideas of the learned professions, Dalton seems to have at once begun a regular series of observations on meteorological phenomena which extended over the years 1790 to 1793. From the year 1787 he had been engaged in investigations of a similar nature, but during the later time he seems to have decided upon a definite course of experiment and observation, the results of which were published in 1793 under the title of *Meteorological Observations and Essays*. Dr. Angus Smith speaks of the book in the following terms:—

"The work seems to have been at first intended as a popular treatise on meteorology. It begins with a description, of the barometer, then come the thermometer, hygrometer and rain gauges; connected with these are tables of observations made at Kendal and Keswick. There seems to be a looseness of description in the first part of the volume, which would seem to imply that the matter was easily understood, and the readers could make out the particulars for themselves. As he proceeds, however, he seems to feel that he has a harder task to perform, and speaks rather to scientific than to popular hearers, whilst we gradually become aware that he is a close and precise reasoner. His style is very

simple; he goes directly to his point; all inessential parts are left out. He seems to move forward with a heavy dogged tread, never turning his head aside, but as any style may become a fault if too far carried out, we find that in his there are left out many things that are certainly needful as accessory or confirmatory, leaving what to the eyes of many is a want of finish, so that others have been needed to complete what was in reality sufficiently complete had it been laid out entire as it existed in his own mind."

The book opens with an entry concerning an aurora which was observed on 27th March 1787. This phenomenon always seemed to excite his interest, for we find later that he speculates as to its nature. A remarkably fine aurora was visible on 13th October 1792, which he describes in the following words:—

"The intensity of the light, the prodigious number and volatility of the beams, the grand admixture of all the prismatic colours in their utmost splendour variegating the glowing canopy with the most luxuriant and enchanting scenery, affording an awful, but at the same time the most pleasing and sublime spectacle in nature. Everybody gazed with astonishment; but the uncommon grandeur of the scene only lasted about one minute; the variety of colours disappeared and the beams lost their lateral motion, and were converted, as usual, into the flashing radiations; but even then it surpassed all other appearances of the aurora, in that the *whole* hemisphere was covered with it."

On study of this he was led to the view that these phenomena were closely connected with terrestrial mag-

netism. Dalton seems to have observed that the aurora had some action on the magnetic needle, and although this had been previously recorded, the observation was apparently quite an independent one, for no mention of the earlier work is made. This failing to make himself thoroughly acquainted with the previous literature of the subject is noticeable more than once in the course of his scientific work, and it can only be attributed to his natural independence of the conclusions of others, and to his desire to rely as far as possible solely upon his own experiments and observations.

The following passage from his *Meteorological Essays* will serve to illustrate his style of writing, and will at the same time show how keen an observer he was and how clearly his inferences were drawn.

"The light of the aurora has been accounted for on three or more different suppositions:—1. It has been supposed to be a flame arising from a chymical effervescence of combustible exhalations from the earth. 2. It has been supposed to be inflammable air, fired by electricity. 3. It has been supposed electric light itself.

"The first of these suppositions I pass by as utterly inadequate to account for the phenomena. The second is pressed with a great difficulty how to account for the existence of *strata* of inflammable air in the atmosphere, since it appears that the different elastic fluids intimately mix with each other; and even admitting the existence of these *strata*, it seems unnecessary to introduce them in the case, because we know that discharges of the electric fluid in the atmosphere do exhibit light, from

the phenomenon of lightning. From these and from other reasons, some of which shall be mentioned hereafter, I consider it almost beyond doubt that the light of the *aurora borealis*, as well as that of falling stars and the larger meteors, is electric light solely, and that there is nothing of combustion in any of these phenomena. Air, and all elastic fluids, are reckoned amongst the non-conductors of electricity. There seems, however, a difference among them in this respect: dry air is known to conduct worse than moist air, or air saturated with vapour. Thunder usually takes place in summer, and at such times as the air is highly charged with vapour; when it happens in winter, the barometer is low, and consequently, according to our theory of the variation of the barometer, there is then much vapourized air; from all which it seems probable, that air highly vapourized becomes an imperfect conductor, and, of course, a discharge made along a stratum of it will exhibit light, which I suppose to be the general cause of thunder and lightning."

With regard to the influence of the aurora on the magnetic needle, Dalton writes as follows:—

"Now, from the conclusions in the preceding sections, we are under the necessity of considering the *beams* of the *aurora borealis* of a ferruginous nature, because nothing else is known to be magnetic, and consequently, that there exists in the higher regions of the atmosphere an elastic fluid partaking of the properties of *iron*, or rather of magnetic steel, and that this fluid, doubtless from its magnetic property, assumes the form of cylindric beams. It should seem, too, that the rainbow-like

arches are a sort of *rings* of the same fluid, which encompasses the earth's northern magnetic pole, like as the parallels of latitude do the other poles; and that the beams are arrayed in equidistant rows round the same pole."

In the light of modern knowledge we may be disposed to smile at the conclusions he reaches with regard to the aurora, but it must be borne in mind that such conclusions were drawn from his own unaided observations, and that he had had practically no help in his work.

Entirely self-taught, living far away from books or men experienced in the scientific methods of the time, it is a matter to excite wonder and admiration that such comparatively accurate work was done under such disadvantageous conditions. Even thus early in his scientific career, Dalton's work seems to give evidence of his striking powers of observation and logical reasoning, and the accuracy of much of his physical work has been repeatedly confirmed. His views as to the causes influencing the rise and fall of the barometer are substantially those held to-day. These he embodied in an essay entitled "On the Variation of the Barometer." In the course of this essay he writes thus :—

"It appears from the observations (recorded in page 15 of the Essays) that the mean state of the barometer is rather lower than higher in winter than in summer, though a stratum of air on the earth's surface always weighs more in the former season than in the latter; from which facts we must unavoidably infer that the height of the atmosphere, or at least of the gross parts

of it, is less in winter than in summer, conformable to the table, p. 80. There are more reasons than one to conclude that the annual variation in the height of the atmosphere, over the temperate and frigid zones, is gradual, and depends in a great measure on the mean temperature at the earth's surface below, for clouds are never observed to be above four or five miles high, on which account the clear air above can receive little or no heat but from the subjacent regions of the atmosphere, which we know are influenced by the mean temperature of the earth's surface; also, in this respect, the change of temperature in the upper parts of the atmosphere must in some degree be conformable to that of the earth below, which we find by experience increases and decreases gradually each year, at any moderate depth, according to the temperature of the seasons.

"Now, with respect to the fluctuations of the barometer, which are sometimes very great in twenty-four hours, and often form one extreme to the other in a week or ten days, it must be concluded, either that the height of the atmosphere over any country varies according to the barometer, or otherwise that the height is little affected therewith, and that the whole or greatest part of the variation is occasioned by a change in the density of the lower regions of the air. It is very improbable that the height of the atmosphere should be subject to such fluctuations, or that it should be regulated in any other manner than by the weekly or monthly mean temperature of the lower regions; because the mean weight of the air is so nearly the same in all the seasons of the year; which could not be if

the atmosphere was as high and dense above the summits of the mountains in winter as it is in summer. However, the decision of this question need not rest on probability; there are facts which sufficiently prove that the fluctuation of density in the lower regions has the chief effect on the barometer, and that the higher regions are not subject to proportional mutations in density. In the *Memoirs of the Royal Academy* at Paris, for 1709, there is a comparison of observations upon the barometer at different places and, amongst others, at Zurich, in Switzerland, in lat. 47° N. and at Marseilles, in France, lat. 43° 15′ N.; the former place is more than 400 yards above the level of the sea. It was found that the annual range of the barometer was the same at each place; viz., about ten lines; whilst at Genoa, in latitude 44° 25′ N., the range was 12 lines, or one inch; and at Paris, latitude 48° 50′ N., it was about 1 inch 4 lines. In the same memoir it is related that *F. Lavel* made observations, for ten days together, on the top of St. Pilon, a mountain near Marseilles, which was 960 yards high, and found that when the barometer varied $2\frac{3}{4}$ lines at Marseilles, it varied but $1\frac{3}{4}$ upon St. Pilon. Now, had it been a law that the whole atmosphere rises and falls with the barometer, the fluctuations in any elevated barometer would be to those of another barometer below it, nearly as the absolute heights of the mercurial columns in each, which in these instances were far from being so. Hence, then, it may be inferred that the fluctuations of the barometer are occasioned chiefly by a variation in the density of the lower regions of the air, and not by

an alternate elevation and depression of the whole superincumbent atmosphere. How we conceive this fluctuation in the density of the air to be affected, and in what manner the preceding general facts relative to the variations of the barometer may be accounted for, is what we shall now attempt to explain."

This is referred to the varying amount of vapour.

Later in these Essays he writes :—" I am confirmed in the opinion that the vapour of water (and probably of most other fluids) exists at all times in the atmosphere, and is capable of bearing any known degree of cold without a total condensation, and that the vapour so existing is one and the same thing with *steam*, or vapour of the temperature of 212° or upwards.

. . . .

" Hence then we ought to conclude, till the contrary can be proved, that the condensation of vapour exposed to the common air, does not in any manner depend upon the pressure of the air."

The Essays on the other subjects are equally remarkable for the same characteristics as those on the aurora and the barometer. The one which may perhaps be counted as of greatest moment is the one on evaporation, for herein is found the nucleus of the idea now known as Dalton's Law which says that the same weight of water vapour is taken up by a given space, whether that space is vacuous or contains air, provided that the temperature of both is the same. More recent and exact measurements show that there are discrepancies in the various observations recorded, more moisture being taken up *in vacuo*, but this may be due

to other causes, in which case the validity of the law would not be impaired.

He showed conclusively that the cause of rain was the lowering of the atmospheric temperature, and in his usual ingenious manner devised a method for determining the dew point. In all his experimental work there seems to have been a certain amount of crudity; his instruments were imperfect, being generally home-made, but in spite of these drawbacks he was able, by means of his natural ingenuity, to conduct experiments, from the results of which he could reason as to the nature of the phenomenon under investigation. This is exceedingly well illustrated by the manner in which he determined the dew-point. He describes his method as follows:—

"The usual mode of my operations was to find a spring on the side of the mountain and then to take a cup of water from it and pour into a clean dry tumbler glass. If dew was produced immediately on the outside of the glass, the water was returned into the cup, and the glass was again carefully dried outside. During this time the water in the cup was acquiring temperature from the air. It was then returned into the tumbler and held out exposed to the current of air. This process was repeated until no dew was found to be formed on the glass. The temperature of the water each time it was put into the tumbler was found by a small pocket thermometer; and *that* when it last produced dew on the glass was marked down as the dew point. At the same time the barometer was noted to find the height of the place of observation, and the thermometer to find

the temperature of the air; and the temperature of the springs was an object not wholly devoid of interest."

As has been said, his diary containing "observations on the weather" was commenced in 1787 and was continued until the day before his death. He seems to have added little to the *Meteorological Essays*, for the second edition, which was published in 1834, contained but little new matter. Speaking of this edition he states that it is practically a reprint "as I apprehend it contains the germ of most of the ideas which I have since expanded more at large in different essays, and which have been considered as discoveries of some importance."

The work we have been considering was published in the autumn of 1793, after Dalton had left Kendal for Manchester.

It was owing to the good services of his friend Gough that he obtained the post of teacher of mathematics in the New College of Manchester, for Dr. Barnes, the then principal, had asked Mr. Gough to recommend a suitable person for the post.

This college was established by the Manchester Presbyterians as a continuation of the Warrington Academy, an institution where another famous English chemist—Priestley—had worked and taught. There were no religious or political tests, so that it was open to men to whom the doors of the older universities were closed. Roscoe in his Memoir on Dalton says:—

"The 'Academy' remained in Manchester until 1803, when it was removed to York, but in 1840 again returned to its original site in Manchester. In 1889

'Manchester College' was again removed, this time to Oxford where stately buildings have been erected next to Mansfield College. Over the entrance to the main building stands the inscription 'To Truth! To Liberty! To Religion!' being the words with which a discourse entitled 'Free Teaching and Free Learning' was concluded by the first principal of the college, Dr. Thomas Barnes, in the inaugural address delivered on 14th September 1786."

It was in this college that Dalton lived and worked during the six years following his appointment. His salary was scarcely a princely one, being only £80 for the academic year from September to June, and from this sum £27, 10s. was deducted for rooms and commons, but he was a man of frugal tastes, and no doubt considered himself as passing rich on what to-day seems a small enough remuneration for the teaching of mathematics, natural philosophy and chemistry. His pupils in 1794 numbered twenty-four and in 1799 twenty-two. These he instructed in mathematics, mechanics, geometry, algebra, book-keeping, natural philosophy and chemistry, and it is recorded that the trustees of the college expressed themselves as entirely satisfied with his services, adding that Mr. Dalton was "happy in possessing the respect and attachment of his pupils."

He now had chances of acquiring knowledge, which, previously, had been denied him, for he had a certain amount of leisure which he employed in reading and performing experiments. In a letter to his cousin Elihu Robinson, he describes his work and mode of life

in terms which make us believe that his day was fully occupied. His letter is sufficiently full of detail to show us what manner of life he lived.

"Our academy," he writes, "is a large and elegant building in the most elegant and retired street of the place. It consists of a front and two wings; the first floor of the front is the hall, where most of the business is done; over it is a library with about 3,000 volumes; over this are rooms, one of which is mine; it is about eight yards by six, and above three high, has two windows and a fireplace, is handsomely papered, light, airy, and retired; whether it is that philosophers like to approach as near to the stars as they can, or that they choose to soar above the vulgar into a purer region of the atmosphere, I know not; but my apartment is full ten yards from the earth.

"One of the wings is occupied by Dr. Barnes' family; he is one of the tutors, and superintendent of the seminary; the other is occupied by a family who manage the boarding, and seventeen in-students with two tutors, each individual having a separate room, etc. Our out-students from the town and neighbourhood at present amount to nine, which is as great a number as has been since the institution; they are all of religious professions, one Friend's (Quaker) son from the town has entered since I came. The tutors are all Dissenters. Terms for in-students, 40 guineas per session (10 months); out-students 12 guineas. Two tutors and the in-students all dine, etc., together in a room on purpose. We breakfast on tea at 8½, dine at 1½, drink tea at 5 and sup at 8½; we fare as well as it is possible for any-

one to do. At a small extra expense we can have any friend to dine with us in our respective rooms. My official department of tutor only requires my attendance upon students 21 hours in the week; but I find it often expedient to prepare my lectures previously.

"There is in this town a large library, furnished with the best books in every art, science and language, which is open to all gratis; when thou art apprised of this and such-like circumstances, thou considerest me in my private apartments, undisturbed, having a good fire, and a philosophical apparatus around me, thou wilt be able to form an opinion whether I spend my time in slothful inactivity of body and mind. The watchword of my retiring to rest is 'Past—12 o'clock—cloudy morning.' Now that I have mentioned clouds, it leads me to observe that I continue my meteorological journal, have two rain-gauges about a mile off at a friend's house; one gauge is in the garden, and the other upon the flat roof of his house, 10 yards higher than the former. I find that the lower gauge catches 12 parts of rain for the upper 11. From my correspondence with my brother, it appears they have had about twice the rain we have. I hope my friends there are not altogether disappointed with my essays; please to make the following correction, and intimate it occasionally to such as have them. Page 37—total rain at Kendal 1790 should be 62·363 and for 1791, 66·200.

"Among my late experiments, have had some on the artificial production of cold, but have not been able to freeze quicksilver. I find that two parts of snow and one of common salt, mixed and stirred, produce a

cold regularly of—7° or 7° below 0. I have sunk the thermometer below 0, in a common wine-glass half-filled with the mixture.

"There is a very considerable body of Friends (Quakers) here; near 200 attend our first-day (Sunday) meetings. I have received particular civility from most of them and am often at a loss where to drink tea on a first-day afternoon, being pressed on so many hands. One first-day lately, I took a walk in company with another to Stockport; there are but few Friends there, but the most elegant little meeting-house that can be conceived; the walls and ceiling perfectly white; the wainscot, seats, gallery, etc., all white as possible; the gallery rail turned off at each end in a fine serpentine form; a white chandelier; the floor as smooth as a mahogany table, and covered with a light-red sand; the house well lighted, and in as neat order as possible; it stands upon a hill; in short, in a fine sunny day it is too brilliant an object to be attended, by a stranger at least, with the composure required. JOHN DALTON."

Such then was his life for six years, years which doubtless helped in no small measure towards those habits of observation and inference which served to make him one of the most noteworthy of English philosophers. It is easy to imagine how great must have been his delight at having at his disposal a well-stocked library and the means for prosecuting his researches, for until he came to Manchester, his opportunities for acquiring information other than by his own unaided observation, were of the most meagre descrip-

tion. His work was still of the most varied kind. In 1797, writing to his brother, he says, "My time at present is much taken up with tuition at home and in the town together; so that I can scarcely turn to any particular and mathematical or philosophical pursuit; but of late I have been attending to the philosophy of grammar and to that of sound."

The result of this attention was a work on English Grammar which appeared in 1801. This he dedicated to John Horne Tooke, M.P., whose writings he greatly admired and to whom he sent a copy. Tooke was arraigned on a charge of high treason, and Dalton, referring to this, writes to his brother saying, "But he (Tooke) has got *things* to attend to now instead of words."

The book was not apparently a great success, although a second edition was issued in 1803. It was concisely written, and contained certain innovations such as the inclusion of the articles with adjectives and a rather surprising view of "tense." While admitting three tenses, viz., past, present and future, from a grammatical point of view, he argues that strictly speaking and looking at the matter from a mathematical point of view, there can be only two, past and future. He says, "It may be taken as an axiom that all time or duration, in the strict sense of the term, is either past or future. But for the purposes of speech we must have a present time of some duration, which must necessarily be composed of a portion of past and a portion of future, having the present *now* or *instant* as a boundary between them."

His own opinions concerning the value of the book are quite decided and are recorded in a letter written to Robinson in 1802 in which he says, "I think, upon the whole, it is as good as any that has succeeded it, but at the same time I think they are all very bad, or I should not have been at the trouble to write one principally for my own use. I am now in the practice of teaching it, and find it the most intelligible to my young people of any they have met with."

In 1797, accompanied by a Quaker friend, he started upon a tour, some incidents of which he describes in the following letter to his cousin Elihu Robinson.

"MANCHESTER, 1st *month*, 27th, 1798.

"DEAR COUSIN,—It is now three months since I received thy kind notice of my letter of last summer. My engagements of teaching, in public and private, together with my own literary pursuits and the necessity of frequent visits amongst an extensive acquaintance, occupy my time so regularly from eight in the morning to twelve at night, that I rarely find an opportunity for occasional correspondence. However, I mean herein to give thee a further account of our tour, agreeable to thy request; only I am afraid that some part will only be *a tale twice told*, as I am not aware of what I wrote last.

"We had a very pleasant passage across the Mersey from Liverpool towards Chester (about twelve miles), and had a fine view of Beeston Castle (about thirty miles), whether we were aiming; we reached Chester in the canal boat about five, and having drank tea, started on foot for Tarporley (ten miles), anticipating the twofold

pleasure of a fine view from the castle the next day, and of there partaking of a cold collation in the open air in company with my amiable friend Eliza Rothwell and her daughters, who were on a visit. They had, however, been suddenly and unexpectedly called home the day before, but had taken care to secure us a welcome reception at their friend's house, which was situated on a hill about three miles from the castle, and in full view of it, a valley intervening. In the morning we had no sooner drawn aside the curtains, than the rising sun shone in upon us, and discovered the most elegant lodging room I was ever in. But that was not all; the views from the windows on two sides of the room were exquisite; we seized upon a large reflecting telescope and pointed it to the castle before we were dressed. After spending the morning there we went over to the castle, which answered our expectation, and then proceeded to Whitchurch that night. Rose at six, and would go to Wem to breakfast (eleven miles); when we had gone two, came to a village where we were told to inquire for a foot road which was about three miles, and said to be at least a mile nearer; there were many cross roads at the village, and we asked at a flax shop on our *left*, which was the short road, and were directed to turn to our left at a barn, etc., etc., and found all was as told, presuming, however, that the main road was right forward; but it happened to be a road still more to the left, as we found to our cost in the sequel. An hour after we got into the main road at right angles to our last track, and turned of course to the left; soon after came to a stone, but its inscription defaced. We rested

awhile, and a person came up who told it was four miles to Wem; unfortunately we were *standing still* when we asked. We proceeded, and another stone presented itself, likewise defaced, which we called three, and going on we began to look long for two, when we entered a village where were many cross roads. My companion, impatient for his breakfast, would inquire of somebody, and stepped aside to a shop on his *right*, whilst I went up to a guide post. I had not got up to it before I heard a voice behind me:—'*We have been here before this morning.*' I went on,—it repeated, '*I say, we have been here before this morning.*' 'What dost thou mean?' said I, turning round; '*Well, I say we have been here before this morning, this is the flax shop I inquired at before, and yonder is the barn.*' I perceived it was so. The very same men that had directed us before, came out, and seemed as much surprised as we, inquiring whether we had been at Wem, as we had asked of them the way about two hours before. Our surprise and chagrin may be easily conceived. They told us to go back the way we had last come, and then they defied us to get wrong; which we did accordingly. In this manner we paid for a piece of advice, 'Never to leave the main road without knowing well on which hand you have it.' We could get nothing but *bread and water* till we got to Wem at eleven, and then we had each about eight or ten cups of coffee."

In this letter he proceeds to describe the rest of the tour through some of the Midland counties. They came to Oxford where he says, "We had a line to one of the Fellows, who showed us what was worth attention at

that celebrated place, as the libraries, gardens, buildings, etc." From Oxford they passed through Slough, Windsor and Eton on their way to London. "The observations made in the metropolis, I must omit," he writes, "as they would require some room." Their route from London was *via* Salisbury and Bristol to Ross and Hereford. Always observant, he records particulars of the scenery and in one place discovers "a profusion of plants we had never before seen, and several of the more rare ones which we had seen."

The letter is a long one, full of details concerning places and their customs. For example he writes, comparing Wales and Cumberland, "In Cumberland every other man one meets has a little estate which he cultivates himself, and enjoys the produce; but in Wales they are *all* labourers, and the masters are never seen, they are not in the country. How can a 'Philanthropic Philosopher' observe these things without emotion?"

In the winter of the same year he revisited his own lake country, staying at Kendal and at Ulverston.

His affection for his own country and people was always a strong one, and occasionally one comes across little touches of sentiment which are the more striking as being unexpected in a man of a somewhat austere nature. As an illustration of this may be mentioned his requesting his cousin to send him a small spinning-wheel from Cumberland. "The one we have got," he says, "gives great satisfaction, and I have the pleasure of seeing it in motion, whilst I smoke my pipe, two or three evenings in the week, though it is more than half a mile from my lodgings. It reminds me of some

pleasant evenings spent at Eaglesfield in times of old, and prevents me repining at the loss of them."

In the same letter he mentions his financial affairs and appears to be comparatively well satisfied. He says :—

"My academy has done very well for me hitherto. I have about eight or nine day pupils at a medium, at ten guineas per annum and am now giving upwards of twenty lesson per week, privately, at two shillings each besides. [I] am not yet rich enough to retire, notwithstanding."

Such then was the manner of Dalton's life during his early years in Manchester. It was only subsequent to this period that his real scientific work began, or perhaps it is more correct to say that the work by which he is best known to men of science of to-day was performed after the period of his life which has been described above.

CHAPTER III

EARLY SCIENTIFIC PAPERS—COLOUR VISION

We have now to consider that period of Dalton's life which Angus Smith calls his epoch of greatest fertility. It was during this epoch that his attentions were directed more especially to those subjects which led him to enunciate his views on the nature of matter, views which have made his name a household word amongst scientists.

In Dalton's case, as in that of so many other workers in scientific fields, his earlier work had apparently no very direct bearing upon those problems which, in later life, he attempted to solve. It would seem that at first he undertook whatever investigation lay nearest to his hand, while later his energies were concentrated on the enunciation and development of the hypothesis which bears his name. At the same time it must be remembered that from his early meteorological work he obtained ideas as to the nature of gases or "elastic fluids," and these were subsequently expanded by him and formed the basis of his most important scientific work.

In spite of the change from the quiet, uneventful life at Kendal to the busy existence in a large town, he continued his old habits of observation—he still remained the earnest student of nature and her laws, so

that we find him in 1794 considered worthy of election as a member of the Literary and Philosophical Society of Manchester. He was proposed by Dr. Thomas Henry, Dr Percival and Robert Owen, and on 3rd October 1794 he was elected a member. His connection with the Society lasted throughout his life. He held successively the posts of Secretary, Vice-president, and President, and contributed to it many memoirs on various subjects.

His first paper to the Society was read one month after his election, the title being

"EXTRAORDINARY FACTS
relating to the
VISION OF COLOURS
with observations
By Mr. JOHN DALTON."[1]

It is probable that prior to 1794 Dalton was aware that he possessed a defective colour sense, but it was in February of that year that he first alluded to the fact in a letter to Elihu Robinson. In this letter he says:—

"I am at present engaged in a very curious investigation. I discovered last summer with certainty, that colours appear different to me to what they do to others. The flowers of the Cranesbills appear to me in the day almost exactly sky-blue, whilst others call them deep pink; but happening once to look at one in the night by candle-light I found it of a colour as different as possible from daylight; it seemed then very near yellow, but with a tincture of red; whilst nobody else said it differed from the daylight appearance, my brother

[1] Memoirs, Lit. and Phil. Soc., Manchester, Vol. V., p. 28.

excepted, who seems to see as I do. . . . The primary colours, *orange*, *yellow*, and *blue*, appear to me much the same in the night as they do in the day, and I always distinguish them and call them by their proper names, as well as several drabs, and other mixed colours; some reds—for instance vermilion—appear the same or alike day and night; but others, and more especially the different shades of *pink*, confound me most completely in the day, they all appearing *light blue;* all the dyed *greens* seem to have little or no green about them; they appear inclining to *red* or *brown* in the day, and almost *blue* in the night; the *pinks* and *light blues*, which appear almost of the same piece in the day, are as opposite as black and white in the night, or by candlelight."

Several stories are told of Dalton illustrating this curious defect, showing how it led him into difficulties. On one occasion, he gave his mother as a birthday present a pair of silk stockings of a brilliant scarlet hue. She acknowledged the gift by saying "Thou hast bought me a pair of grand hose, John, but what made thee fancy such a bright colour? Why, I can never show myself at meeting in them!" To which John replied that he regarded them as of a dark-bluish drab colour of an eminently respectable go-to-meeting shade. Evidently, her son was hard to convince on the subject, for she consulted her neighbours, and as a result expressed the opinion that they were "varra fine stuff; but uncommon scarlety."

Again in 1821, when about to visit Paris, he considered that the occasion was sufficiently important to justify the purchase of a new suit of clothes and accordingly went to

a Manchester tailor with a request for "some good strong drab cloth." A piece lying before him caught his eye, and he remarked, "I think this will suit, just the colour I want, and stout good cloth." To which the tailor replied that it was a piece of scarlet cloth for hunting coats! The unfortunate man, seeking his sober Quaker drab, replied, "Ah! I see thou knowest the infirmity of my eyes."

As soon as he was aware of this defect in his vision, he started, in characteristic fashion, to collect information on the subject. In 1777 a Captain Huddart had written a letter to Priestley concerning the abnormal eyesight of a Maryport family named Harris. In a letter to his "Respected Friend, Joseph Dickinson" of Maryport, Dalton says: "I lately read Huddart's account of the Harrises in the *Philosophical Transactions* for 1777, but it does not satisfy me." He accordingly asks Dickinson to interview Captain Harris, or (in case of his absence) some of the relatives, and to report to him as soon as possible the answers to the following series of questions.

"Query 1. Did he ever look through a prism? What are the chief colours he sees in it?

"2. Do not *pinks*, *roses*, etc., which others call *red*, appear to him to have some affinity to sky-blue?

"3. Has he distinct ideas of *red*, *orange*, *yellow*, *green*; or does he not meet with colours which he would hesitate to pronounce one of these rather than another?

"4. What are the most conspicuous colours of the rainbow?

"5. Does the green woollen cloth, used to cover

tables, etc., appear *green*, or any way like grass to him ; or would he not call it a *brownish-red?* Whether is common red *sealing-wax* or it more nearly the colour of grass ?

" 6. I wish particularly to know whether a ribband of a *deep pink* colour appears remarkably different by *day*-light and *candle*-light, as well as *dark green* and *crimson?*

" 7. Does he perceive in the daytime much difference between *crimson* and *dark drab?*"

These questions were answered by two brothers other than those mentioned by Huddart, who also were colour-blind, and they appear to have satisfied Dalton that the defect was similar to his own.

Pinks and roses seemed to them sky-blue ; the most conspicuous colour of the spectrum was yellow ; the green tablecloth was not of the same colour as grass, and red sealing-wax seemed rather darker ; no distinction could be drawn between dark green and the colour of blood ; the colour of a pink ribbon seemed totally different by artificial light—in short, as Dickinson says in reply to Dalton's enquiries, " he has no *just ideas* of any colour except *black*, *white*, and *yellow*."

Later Dalton sent to his " respected friend," a number of coloured ribbons, to each of which he had assigned a colour as it appeared to him first by daylight and then by candle-light. He requested him to make Harris record his colour impressions so that he might be able to compare them with his own. He says :—

" Now, if thou hast read these remarks with the colours before thee, and hast kept a grave face all the while, thou hast done more than anybody here has yet ;

but the person thou art to show the colours to will find nothing strange in all this, I expect."

In reply Dickinson laughs at Dalton and indulges in some good-natured chaff on the subject. He writes :—

"Indeed, friend John, thou conjectured right; I did not read thy remarks with a grave face, but on the contrary with many fits of risibility which I am subject to, but I think more so on hearing J. Harris' remarks and my own reflections thereon. I find by your accounts you must have very imperfect ideas of the charms which in a great measure constitute beauty in the female sex, I mean that rosy blush of the cheeks, which you so much admire for being *light blue*, I think a complexion nearly as exceptional as the sun-burnt Moor's or the sable Ethiopian's, consequently (if real), a fitter object for a show than a wife."

The observations made on himself and the results of his inquiries into the case of the Harris family were sufficient to enable him to write a Memoir on the subject. This, as already stated, was his first communication to the Manchester Literary and Philosophical Society.

In this paper, he shows distinctly his powers as an acute observer, and the conclusions which he draws, though in some cases erroneous, show that he was also a clear and logical reasoner. His style, of which this example is fairly typical, is somewhat abrupt; there are no unnecessary statements; his facts are all in order and he brings them forward plainly and in the most condensed form. His inferences are made in the same direct manner and the impression obtained from reading

his works is that the writer was a simple man, who said what he had to say in as few words as possible—in short, that it was the substance that was the important part, and the outer dress of words only useful in so far as it served to convey its meaning to the reader. A few extracts may help to illustrate this, and will be of interest as forming part of his first contribution to scientific literature, if we except his meteorological work.

After a short preamble, plunging as was his wont into the heart of things, he says :—

"I was always of opinion—though I might not often mention it — that several colours were injudiciously named. The term *pink*, in reference to the flower of that name, seemed proper enough; but when the term *red* was substituted for pink I thought it highly improper. It should have been *blue*, in my apprehension, as pink and blue appear to me very nearly allied, whilst pink and red have scarcely any relation.

"In the course of my application to the sciences, that of optics necessarily claimed attention, and I became pretty well acquainted with the theory of light and colours before I was apprised of any peculiarity in my vision. I had not, however, attended much to the practical discrimination of colours, owing, in some degree, to what I conceived to be a perplexity in the nomenclature. Since the year 1790, the occasional study of botany obliged me to attend more to colours than before. With respect to colours that were *white*, *yellow* or *green*, I readily assented to the appropriate term. *Blue*, *purple*, *pink*, and *crimson*, appeared rather less dis-

tinguishable, being, according to my idea, all referable to *blue*. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest. Notwithstanding this, I was never convinced of a peculiarity in my vision till I accidentally observed the colour of the flower of the *Geranium zonale* by candle-light in the autumn of 1792. The flower was pink, but it appeared to me almost an exact sky-blue by day; in candle-light, however, it was astonishingly changed, not having then any blue in it, but being what I called red—a colour which forms a striking contrast to blue."

He goes on to state that he requested friends to observe this phenomenon and found to his surprise that the colours as seen in the different lights were not markedly different from one another. His brother, however, saw the difference. He further informs us that two years after this discovery he commenced a serious investigation of the subject, and continues as follows:—

"I shall now proceed to state the facts ascertained under the three following heads:—

"I. An account of my own vision.

"II. An account of others whose vision has been found similar to mine.

"III. Observations on the probable cause of an anomalous vision.

"I. Of My Own Vision.

"It may be proper to observe that I am short-sighted. Concave glasses of about five inches focus suit me best.

I can see distinctly at a proper distance, and am seldom hurt by too much or too little light, nor yet with long application.

"My observations began with the spectrum, or coloured image of the sun, exhibited in a dark room by means of a glass prism. I found that persons in general distinguish six kinds of colour in the solar image—namely, red, orange, yellow, green, blue, and purple. My yellow comprehends the red, orange, yellow, and green of the others; and my blue and purple coincide with theirs. That part of the image which others call red appears to me little more than a shade, or defect of light; after that, the orange, yellow, and green seem one colour, which descends pretty uniformly from an intense to a rare yellow, making what I call different shades of yellow. The difference between the green part and the blue part is very striking to my eye: they seem to be strongly contrasted. That between the blue and purple is much less so. The purple appears to be blue much darkened and condensed. In viewing the flame of a candle by night through the prism the appearances are pretty much the same, except that the red extremity of the image appears more vivid than that of the solar image.

"I now proceed to state the results of my observations on the colours of bodies in general, whether natural or artificial, both by daylight and candle-light. I mostly used ribbands for the artificial colours.

"*Red (by Daylight)*.

"Under this head I include *crimson*, *scarlet*, *red*, and

pink. All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud* which were very nearly alike. Crimson has a *grave* appearance being the reverse of every shewy and splendid colour. Woollen yarn dyed crimson or dark blue is the same to me. *Pink* seems to be composed of nine parts of light blue and one of red, or some colour which has no other effect than to make the light blue appear dull and faded a little. Pink and light blue, therefore, compared together, are to be distinguished no otherwise than as a splendid colour from one that has lost a little of its splendour. Besides the pinks, roses, etc., of the gardens the following British *flora* appear to me blue—namely, Statice Armeria, Trifolium pratense, Lychnis Flos-cucula, Lychnis dioica, and many of the Gerania. . . . The colour of a florid complexion appears to me that of a dull, opake, blackish-blue upon a white ground. A solution of sulphate of iron in the tincture of galls—that is, dilute black ink—upon white paper, gives a colour resembling that of a florid complexion. It has no resemblance to the colour of blood. *Red* and *scarlet* form a genus with me totally different from pink. My idea of red I obtain from *vermilion, minium, sealing-wax, wafers, a soldier's uniform*, etc. These seem to have no blue whatever in them. Scarlet has a more splendid appearance than red. Blood appears to me red, but it differs much from the articles mentioned above: it is much more dull, and to me is not unlike that colour called bottle-green. Stockings spotted with blood or with dirt would scarcely be distinguishable.

"*Red (by Candle-light).*

"Red and scarlet appear much more vivid than by day. Crimson loses its blue and becomes yellowish red. Pink is by far the most changed—indeed it forms an excellent contrast to what it is by day. No blue now appears: yellow has taken its place. Pink by candle-light seems to be three parts yellow and one red, or a reddish yellow. The blue, however, is less mixed by day than the yellow by night. Red, and particularly scarlet, is a superb colour by candle-light; but by day some reds are the least showy imaginable—I should call them dark drabs."

After reading this it is quite easy to understand how the sober-minded Quaker, caring only for quiet shades in clothing, came to select scarlet stockings for his equally sober-minded Quaker mother, and hunting scarlet for his own dress.

He continues his observations on the colours as seen by him and his brother in ordinary and artificial light. They may be briefly summarised as follows:—Orange and yellow are seen much as by persons of normal vision; green—for which he takes grass as a standard, is much like sealing-wax; ale, a solution of liver of sulphur, etc., appear green; green woollen cloth appears in colour like a red soil freshly turned by the plough; very light shades of green seem white.

After giving an account of cases similar to his own, he gives a summary of what he calls the "Characteristic Facts of our Vision," the pronoun referring to those who, like himself, were abnormal as regards the colour sense.

"Characteristic Facts of Our Vision.

"1. In the solar spectrum three colours appear—yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

"2. *Pink* appears by daylight to be sky-blue, a little faded; by candle-light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

"3. *Crimson* appears a muddy blue by day, and crimson woollen yarn is much the same as dark blue.

"4. *Red* and *scarlet* have a more vivid and flaming appearance than by daylight.

"5. There is not much difference in colour between a stick of red sealing-wax and grass by day.

"6. Dark green woollen cloth seems a muddy red, much darker than grass and of a very different colour.

"7. The colour of a florid complexion is dusky blue.

"8. Coats, gowns, etc., appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

"9. In all points where we differ from other persons, the difference is much less by candle-light than by daylight."

Finally, he speculates as to the cause of this abnormality; his hypothesis is at least ingenious, though subsequent research has shown that it is quite untenable.

"It appears, therefore," he writes, "almost beyond a

doubt that one of the humours of my eye, and of the eyes of my fellows, is a *coloured* medium, probably some modification of blue. I suppose it must be the vitreous humour, otherwise I apprehend it might be discovered by inspection, which has not been done. It is the province of physiologists to explain in what manner the humours of the eye may be coloured, and to them I shall leave it, and proceed to show that the hypothesis will explain the facts stated in the conclusion of the second part."

The publication of this Memoir aroused general interest in the subject, and a number of investigators began to attack the problem. The curious defect in colour vision has received a variety of names. Wartmann and Prevost called it "Daltonism," Wilson invented the term "Chromato-Pseudopsis," Herschel proposed "Dichromic vision." To each of these there is some objection, though Wilson's name is probably the best. "Dichromic vision" is not sufficiently accurate, for many people who are affected in this way can detect, as could Dalton, differences between red, yellow, and blue, whereas the name would imply that such persons could only distinguish two colours. The phenomenon is generally known to-day by the name of "colour-blindness," a name due to Brewster, which, though not quite free from objection, is a more convenient term for every-day purposes than the more accurate "Chromato-Pseudopsis" of Wilson.

Dalton's hypothesis, that the vitreous humour of the colour-blind eye is a coloured medium, is admitted on all sides to be erroneous. After his death, one of his

eyes was examined by Mr. Ransome (a Manchester surgeon), who found that no appreciable difference from the normal eye could be detected, so that this simple explanation is quite incorrect, the cause being much more obscure and probably connected with nervous phenomena. The theories as to the nature of this abnormality which have been most widely accepted and most fruitful in stimulating research are those of Young, Helmholtz, and Hering. Young held that there were three kinds of nerve fibres in the retina each of which was sensitive chiefly to light of certain wave lengths. He believed that there were three primary colour sensations, red, green, and violet. The physiological synthesis of white light is effected by the equal and simultaneous stimulation of the three kinds of fibres. Helmholtz developed this view but also held that there were three fundamental colour sensations, red, green, and violet. He thought that certain portions of the retina, which may be called terminals, were excited by light of certain wave lengths. For example, the "red" terminals are stimulated principally by light of short wave length so that a sensation of red is perceived. The "green" and "violet" terminals would not be so affected; but supposing some light of greater wave length fell on the retina, while that of shorter wave length predominated, then the sensation perceived would be a shade of red, the shade depending on the amount of stimulation undergone by the "green" and "violet" terminals. The most frequent form of colour-blindness is the one in which there is inability to distinguish between the

various parts of the red-green portion of the spectrum. This the supporters of the Young-Helmholtz theory would attribute to the failure of either the red or green terminals to respond to selective stimulation. Assuming this, we are at once confronted with the difficulty that the sensation of white light is unimpaired, and it seems hardly probable that this could be so if one set of terminals were defective.

Another hypothesis, due to Hering, assumes the existence of six distinct fundamental sensations arranged in pairs, the pairs being white and black, red and green, yellow and blue. The first-named of each pair he regards as katabolic in function or concomitants of disintegration, the others as anabolic or concomitants of integration. The visual substance of the retina is supposed to be of three kinds each capable of stimulation by one of the three above-mentioned pairs of colours. Hering explains colour sensations as due to the relative amounts of disintegration and integration taking place in the visual substance. Thus, changes in the "white-black substance" give rise to a colour sensation in the brain, the exact colour perceived being dependent upon the amount of destruction and construction occurring in the visual substance. According as disintegration or integration is in excess, white or black, or a certain shade of grey, is the perceived colour. König and von Kries have shown that there are two different types of red-green blindness which cannot be explained by simple defect of the red-green visual substance of Hering.

The question is still unsettled, some facts tending

to support one view, some the other. Recent work seems to confirm to some extent the idea of a limited small number of fundamental colour sensations, as required by the Young-Helmholtz hypothesis, but the evidence obtained from many sources is so conflicting as to render any definite conclusion impossible.

The subject, to which Dalton's Memoir first directed marked attention, has become of extreme importance in these later days, when travelling by land and sea has increased to such an enormous extent.

At night, red and green lights are used on all our railways as indications of danger and safety respectively, and by day flags of the same colour are used similarly. At sea the same colours are used for the port and starboard lights, so that it is a matter of the highest importance that officials of trains and ships should be properly tested as to their colour sense before allowing them to fill posts where inability to distinguish between red and green might involve the safety of so many.

The method of testing most generally employed is a modification of Dalton's original plan. He used coloured ribbons; to-day the place of these ribbons is taken by skeins of wool, though a somewhat more elaborate method involving the use of large numbers of coloured glasses has been devised. The question, as befits one of such vital importance to the safety of mankind, is receiving its due share of recognition at the hands of those in authority, and it is to be hoped that, before long, stringent regulations as to the testing for colour-blindness may be enforced.

CHAPTER IV

EARLY PHYSICAL AND CHEMICAL RESEARCHES

"Ein Chemiker der kein Physiker ist, ist gar nichts." So said Bunsen, one of the great masters of last century; and surely Dalton must have fulfilled his ideal of a chemist, for he was *par excellence* a physicist who later turned his attention to those problems in connection with which his name has been handed down to posterity.

It was not until about the year 1795 that his attentions were directed towards chemistry. This change may be attributed to his having attended a course of lectures given in Manchester by Dr. Garnet, for he appears to have been impressed by the attraction of the demonstrations given in connection with the lectures. In June 1796 we find him writing to his brother, saying that he had some ideas of giving a course in Kendal during that summer, the course to include six lectures on chemistry and the same number on physics. He says, "twenty subscribers at half-a-guinea would be a sufficient inducement to commence."

In the interval between the reading of his first paper in 1794, and 1799, we have no record of his having published any scientific papers. The explanation probably lies in the fact that his time was sufficiently well occupied in the fulfilment of his duties as teacher. We know that during this period he produced his *Elements of English Grammar*, but it is quite clear that he must

have devoted a considerable time to reading and experiment, for soon after giving up his post at the New College in 1799, he read a second Memoir before the Literary and Philosophical Society. The subject of this shows that he was still strongly attached to his first love—meteorological phenomena—the title of the paper being "Experiments and observations to determine whether the quantity of Rain and Dew is equal to the quantity of water carried off by the rivers and raised by evaporation; with an inquiry into the origin of springs." [1] In this essay he discusses the subject under the following headings:—

"1. Of the quantity of rain and dew.
"2. Of the quantity of water that flows into the sea.
"3. Of the quantity of water raised by evaporation.
"4. Of the origin of springs."

In the course of his paper he introduces ideas concerning aqueous vapour which are of importance from the point of view of his later work on vapours. He shows that water vapour exists as an elastic fluid in the atmosphere but not in a state of chemical combination; that the amount present is dependent on the temperature, and that it is always present, the amount varying according to circumstances.

In his next paper he turns to a subject of a more purely physical nature. Count Rumford had attempted to show that fluids do not transmit heat in the same way that solids do but "circulate it solely by the internal motion of their particles." Dalton on April 12th, 1799,

[1] Mem., Lit. Phil. Soc., Manchester, Vol. V. p. 346.

read a paper entitled, "Experiments and observations on the power of fluids to conduct Heat, with reference to Count Rumford's seventh essay on the same subject."[1]

The physicist of to-day can hardly refrain from smiling at the experimental methods Dalton adopted in this investigation. But crude though his methods were, his results were sufficiently satisfactory to allow of right conclusions. One of his experiments may be quoted in his own words, as an illustration of this statement and of his ingenuity and powers of reasoning.

"Exp. 3. Took an ale glass of a conical figure, 2½ inches in diameter, and 3 inches deep, filled it with water that had been standing in the room, and consequently of the temperature of the air nearly. Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water, then having marked the temperature, I put the red-hot tip of a poker half-an-inch deep into the water, holding it there steadily about half a minute, and as soon as it was withdrawn I dipped the bulb of a sensible thermometer about ¼ inch when it rose in a few seconds to 180°.

"TEMPERATURE.

Time	At Top	Middle	Bottom
Before the poker was immersed . .	—	—	47°
— . .	180°	—	47°
5 min. . . .	100°	60°	47½°
20 ,, . . .	70°	60°	49°
1 hour . . .	55°	—	52°"

[1] Mem., Lit. Phil. Soc., Manchester, Vol. V. p. 373.

As a result of this and other experiments, he comes to the conclusion that the circulation of heat in fluids is due "*principally* to the internal motion excited by an alteration of specific gravity, but not *solely* to that cause as Count Rumford has inferred."

A further experiment consisted in mixing hot and cold water and then after a few minutes taking the temperature at the top and bottom. He found that the upper layers were at the same temperature as the lower ones, and argued as follows:—

"If the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided, so as to have made a material difference in the temperature."

There is in this paper a great wealth of careful reasoning and judicious experimentation which shows plainly how earnest a worker he was, and how minutely he observed conditions and their influence on the phenomena under consideration. He shows that the freezing of water is arrested when contained in capillary tubes; he notices the sudden expansion on solidification, and tries to determine the point of maximum density. This he fixes finally at 38° Fahrenheit, a value which was shown subsequently to be somewhat too low.

In May 1800, Dalton was elected Secretary of the Literary and Philosophical Society of Manchester, which office he held until 1808, when he was made a Vice-President. In 1817, he was raised to the dignity of the Presidential Chair, and continued in this high office for the rest of his life.

His next paper was read on June 27th, 1800, one month after his election to the Secretaryship. The title is "Experiments and Observations on the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air."[1] The most important point in this Memoir is an observation on the expansion of gases by heat. He found that the temperature of air mechanically compressed to one-half its previous volume was raised 50° and he further observed that gases expand $\frac{1}{10}$th of their volume when the temperature is raised 50°.

Perhaps the most important of these earlier physical papers was one entitled, "Experimental Essays on the Constitution of Mixed Gases; on the force of Steam or vapour from water or other liquids in different temperatures, both in a Torricellian vacuum and in air; on evaporation; and on the expansion of gases by heat."[2] This was read in three parts on October 2nd, 16th, and 30th, and contained certain fundamental laws the truth of which has been confirmed by later work. A reservation must be made, however, in the case of the first of these laws; Dalton himself modified the one in question to make it accord with more recently acquired knowledge. As Roscoe points out, this law is the only one which is based upon theoretical considerations, the others being deduced from the results of experiment. The laws are four in number and are stated by him in the following terms:—

"1. When two elastic fluids, denoted by A and B,

[1] Mem., Lit. Phil. Soc., Manchester, Vol. V. p. 515.

[2] *Ibid.*, Vol. V. p. 535.

are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of A do not repel those of B as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same at equal distances above or below the several temperatures at which they boil in the open air, and that force is the same under any pressure of another elastic fluid as it is *in vacuo*. Thus the force of aqueous vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so, likewise, the vapour from sulphuric ether, which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force; and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to ½ inch of mercury when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

"3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

"4. All elastic fluids expand the same quantity by heat; and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°. It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it, and conse-

quently the expansion of the fluid as the cube of the temperature reckoned from the point of total privation."

Here, then, we have a clear and definite statement of his views on the behaviour of gases and vapours. His statements are made as the result of experiment, and no hypothesis is introduced to account for the phenomena. At the same time it is hard to resist the conclusion that even at this early stage, he had come to regard matter as not "mingled and massed into indissoluble union," but as made up of particles each free, yet influenced by and influencing the neighbouring ones. It seems as though he had already arrived at the conception of atoms, which Lucretius had expressed long before.

> "How moving about do the prime material atoms
> Shape forth this thing and that thing; and, once shaped, how they resolve them;
> What power says unto each, This must be; how an inherent
> Elasticity drives them about Space vagrantly onward . . ."

The first of these laws is a partial explanation of the diffusion of gases, but in the form in which it was first stated it is of too general a nature. If true, then the amount of oxygen present in the atmosphere—oxygen being a heavier gas than nitrogen—should diminish as the height increased. Dalton held this view, but experiment has shown that it is not a correct one. In his *New System of Chemical Philosophy* the law was restated in the following form:[1]—

"1. The diffusion of gases through each other is effected by means of the repulsion belonging to the

[1] *New System of Chemical Philosophy*, Part I. p. 191 (1842).

homogeneous particles; or to that principle which is always energetic to produce the dilatation of the gas.

"2. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

His method of determining vapour tension he describes as follows:—

"My method is this: I take a barometer-tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air. The barometer by standing some time exhibits a portion of water, etc., of $\frac{1}{2}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column; because being lighter it ascends by the side of the tube, which may now be inclined, and the mercury will rise to the top, manifesting a perfect vacuum from air. I next take a cylindrical glass-tube open at both ends, and of 2 inches diameter and 14 inches in length, to each end of which a cork is adapted, perforated in the middle so as to admit the barometer tube to be pushed through and to be held fast by them; the upper cork is fixed 2 or 3 inches below the top of the tube, and is half cut away so as to admit water, etc., to pass by, its service being merely to keep the tube steady. Things being thus circumstanced

water of any temperature may be poured into the wide tube and thus made to surround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be observed from the depression of the mercurial column. In this way I have had water as high as 155° surrounding the vacuum; but as the higher temperatures might endanger a glass apparatus, instead of it I used the following:—

"Having procured a tin tube of 4 inches in diameter and a foot long, with a circular plate of the same soldered to one end, having a round hole in the centre like the tube of a reflecting telescope, I got another smaller tube of the same length soldered into the larger, so as to be in the axis or centre of it; the small tube was open at both ends, and on this construction water could be poured into the large vessel to fill it, whilst the central tube was exposed to its temperature. Into this central tube I could insert the upper half of a syphon barometer, and fix it by a cork, the top of the narrow tube also being corked; thus the effect of any temperature under 212° could be ascertained, the depression of the mercurial column being known by the ascent in the exterior leg of the syphon. The force of vapour from water between 80° and 212° may also be determined by means of an air pump, and the results exactly agree with those determined as above. Take a Florence flask half-filled with hot water, into which insert the bulb of a thermometer, then cover the whole with a receiver on one of the pump plates and place a barometer gage on the other; the air being slowly exhausted, mark both the thermometer and barometer

at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the observed temperature. This method may also be used for other liquids. It may be proper to observe the various thermometers used in these experiments were duly adjusted to a good standard one."

In this manner, he was able to prepare a table of the vapour pressures, or "the force of steam from water" as he calls it, between the freezing and boiling points.

In his fourth essay he discusses the expansion of gases by heat, points out the causes of error in the values previously obtained, and attempts to exclude these in his own experiments. He examined the behaviour of atmospheric air, hydrogen, oxygen, carbonic acid, and nitrous gas, and found that they all obeyed the same law of expansion. His conclusions are as follows:—

"This remarkable fact that all elastic fluids expand the same quantity in the same circumstances, plainly shews that the expansion depends solely upon heat, whereas the expansion in solid and liquid bodies seems to depend upon an adjustment of the two opposite forces of heat and chemical affinity: the one a constant force in the same temperature, the other a variable one according to the nature of the body—hence the unequal expansion of such bodies. It seems, therefore, that general laws respecting the absolute quantity and nature of heat are more likely to be derived from elastic fluids than from other substances."

These four essays are undoubtedly of the highest importance, not only because they mark a distinct

advance in scientific knowledge, but also because they indicate clearly the direction in which Dalton's thoughts were tending, and serve to illustrate his methods of investigation. Writing of these essays, Dr. Henry in his *Life of Dalton* says :—

"His instruments of research, chiefly made by his own hands, were incapable of affording accurate results, and his manner of experimenting was loose, if not slovenly. His numerical determinations have not, therefore, like even the earlier analyses of Prout, been confirmed by subsequent inquiries. Still his experiments, though wanting in the exactitude of modern research, were not unskilfully devised, and were most sagaciously interpreted.

"They were, perhaps, such as were most needed at the close of the last century, when so many fields of experimental research were untilled, that bold tentative incursions into new domains of thought, large groupings, and happy generalizations of approximate results were more effective instruments of advance than scrupulous precision in details."

It should be noted that, although the law of expansion of gases by heat is generally known by the name of "Gay Lussac's Law," Dalton undoubtedly has the right of priority; for his results were published six months earlier than those of the French chemist, and although these latter more closely approximate to the values at present accepted, yet Dalton's values only differ from them to a trifling extent.

On November 12th, 1802, he read a paper entitled "On the Proportion of the several Gases or Elastic

Fluids, constituting the Atmosphere; with an Inquiry into the Circumstances which distinguish the *Chymical* and *Mechanical* absorption of Gases by Liquids." [1]

His analysis is given for volume and for weight. He finds that the proportions of oxygen and nitrogen are fairly constant, a fact not in accordance with the results of previous workers (unless those of Cavendish be excepted), and this want of agreement he assigns to their ignorance of the nature of the operation. The method he employed was the eudiometric one of mixing the air with nitric oxide ("nitrous gas") over water and observing the decrease in volume, and the results are sufficiently striking to justify quotation.

"If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10ths of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

"If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

"If in the last experiment less than 72 measures of nitrous gas be used, there will be a residuum containing oxygenous gas; if more, then some residuary nitrous gas will be found."

"These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion,

[1] Mem., Lit. Phil. Soc., Manchester (New Series), Vol. I. p. 244.

but with no intermediate quantity. In the former case *nitric* acid is the result, in the latter *nitrous* acid; but as both these may be formed at the same time, one part of the oxygen going to one of nitrous gas and another to two, the quantity of nitrous gas absorbed should be variable; from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject."

These results, together with those found in his next paper, are of interest chiefly as showing the earliest stages of his conception of the Atomic Theory and the laws of combination. The idea of the law of multiple proportions underlies his explanation of the failure to obtain constant values in the above analyses, and we feel that we are on the verge of the great hypothesis by means of which he sought to explain the laws of combination.

He further prosecuted his inquiries into the behaviour of gases, and soon after the publication of the last paper we find one, read on January 28th, 1803, "On the tendency of elastic fluids to diffusion through each other." [1]

In this investigation he takes up the question of diffusion at the point at which it had been left by Priestley. The latter had shown that gases were able to diffuse through a porous medium, but he failed to realize the complete nature of the phenomenon of diffusion. He suggested that as a layer of oil will remain indefinitely on the surface of water, so "if two

[1] Mem., Lit. Phil. Soc., Manchester, Vol. I. (New Series), p. 259.

kinds of air of very different specific gravities were put into the same vessel with very great care, they might continue separate."

To settle this point Dalton had recourse to a very simple experiment. He used two phials connected by a narrow tube; the whole apparatus was set up in a vertical position. In the lower phial was placed the heavier gas, in the upper one the lighter. The gases used were, oxygen, hydrogen, nitrogen, carbonic acid, and nitrous oxide. After the apparatus had remained at rest for a certain length of time, the contents of each phial were analysed, the invariable result being that diffusion had occurred.

The quantitative aspect of the question was neglected by Dalton and subsequent workers, until Graham again took up the subject and established the law which to-day bears his name, viz., that gases diffuse at a rate which is inversely proportional to the square roots of their densities. In the light of modern knowledge it is interesting to notice how shrewd a guess Dalton made as to the nature of diffusion. He says:—

"1. The diffusion of gases through each other is effected by means of the repulsion belonging to the homogeneous particles, or to that principle which is always energetic to produce the dilatation of the gas.

"2. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel, or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

This, after all, may be legitimately regarded as an

anticipation of the dynamical theory of gases which has been so great a help to the understanding of the laws which seem to govern the motion of the molecule.

In his next research Dalton turned his attention to the phenomena of the solubility of gases in water, and the paper in which he recorded his results—"On the absorption of Gases by Water and other Liquids" [1]—must be looked upon as an epoch-making one, for it is here that we find the first distinct indication of his great theory.

Henry had communicated a paper to the Royal Society containing results which showed that the amount of gas absorbed by water varies directly as the pressure. This law is now known as Henry's Law, and it was doubtless after reading of it, or possibly through actual conversation with Henry (for the two men of science were personal friends), that Dalton interested himself in the subject. Whereas Henry had only examined the behaviour of one gas at a time as regards its solubility, Dalton investigated the behaviour of more than one, and established the law now known as Dalton's Law of Partial Pressures. He found that on shaking a mixture of two gases—say oxygen and nitrogen—the amount of each dissolved is the same as if the other were absent, that is to say each constituent dissolves according to its partial pressure. Dalton's own words are :—

"If a quantity of water free from air be agitated with a mixture of two or more gases, such as atmospheric air, the water will absorb portions of each gas the same as if they were presented to it separately in their proper density."

[1] Mem., Lit. Phil. Soc., Manchester, Vol. I. (New Series), p. 271.

"*Ex. gr.*—Atmospheric air, consisting of 79 parts azotic gas, and 29 parts oxygenous gas, per cent., water absorbs $\frac{1}{64}$[1] of $\frac{79}{100}$ azotic gas $= 1{\cdot}234$; $\frac{1}{27}$ of $\frac{21}{100}$ oxygen gas $= {\cdot}778$. Sum, per cent. $= 2{\cdot}012$."

He offers a theory to account for the absorption of gases, and some of his conclusions may be given here.

"1. All gases that enter into water and other liquids, by means of pressure, and are wholly disengaged again by the removal of that pressure, are mechanically mixed with that liquid, and not chemically combined with it.

"2. Gases so mixed with water, etc., retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

"3. Each gas is retained in water by the pressure of its own kind abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect."

Earlier in the paper he had stated that gases are absorbed by water according to certain definite mathematical laws. Either water absorbed its own bulk of gas or else $\frac{1}{8}$th, $\frac{1}{27}$th, $\frac{1}{64}$th, and so on, these fractions being the cubes of the reciprocals of the numbers 1, 2, 3, 4, etc.

This view is entirely an incorrect one, and it is rather

[1] These factors $\frac{1}{64}$ and $\frac{1}{27}$ represent respectively the coefficients of absorption of nitrogen and oxygen, according to the arbitrary rule of Dalton mentioned below.

surprising that Dalton should have put forward such an hypothesis without submitting it to the test of experiment.

His theory of the mode of solution is most mechanical, but it is an indication of the firm hold on his imagination which the idea of particles had obtained. He gives pictures illustrating the position of the dissolved particles in "perpendicular and horizontal strata," and according to his supposed law of fractions he shows how the distances between these particles may be determined. Then follows what can only be regarded as the most valuable part of the paper—at any rate from the point of view of his atomic theory.

"The greatest difficulty attending the mechanical hypothesis arises from different gases obeying different laws. Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and although I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases; those whose particles are lightest being least absorbable, and the others more, according as they increase in weight and complexity. (Subsequent experience renders this conjecture less probable). An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new; I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper, but I shall just subjoin the results, as far as they appear to be ascertained by my experiments.

"TABLE OF THE RELATIVE WEIGHTS OF THE ULTIMATE PARTICLES OF GASEOUS AND OTHER BODIES.

Hydrogen	1	Nitrous oxide	13·7
Azot	4·2	Sulphur	14·4
Carbone	4·3	Nitric acid	15·2
Ammonia	5·2	Sulphuretted hydrogen	15·4
Oxygen	5·5	Carbonic acid	15·3
Water	6·5	Alcohol	15·1
Phosphorus	7·2	Sulphureous acid	19·9
Phosphuretted hydrogen	8·2	Sulphuric acid	25·4
Nitrous gas	9·3	Carburetted hydrogen from stagnant water	6·3
Ether	9·6	Olefiant gas	5·3"
Gaseous oxide of carbone	9·8		

CHAPTER V

DALTON'S CHARACTER AND SOCIAL LIFE

BEFORE proceeding to describe the later work of Dalton, including his *magnum opus*, the work in connection with the Atomic Theory, it will be convenient to turn aside for a time from the man of science as revealed in his memoirs, and to see him as he appeared to his friends and acquaintances.

Lonsdale in his life of Dalton sums up the uneventful character of his life in the following terms.

"There are but faint tracings of historical interest in the life of a man who had to pursue the calling of a schoolmaster, and to practise the sedate virtues of a bachelor Quaker, whose walk was mainly confined to a circle neither enlivening in tone nor brilliant in social qualities. Dalton's life was truly in his works, his science, and his discoveries; in any other direction it was monotonous in form and detail."

As will be seen in the following brief account of his daily life, his habits were of the most regular and his tastes of the simplest kind. In his recreation—for he had apparently but one—there is to be observed the same precision, the same orderly habit of mind as is noticeable in his scientific work. He was, in short, a man whose life ran in a groove, who disliked change. With regard to politics and social matters, he like Gallio of old "cared for none of these things."

In appearance he was of about the middle height, and in build a true son of the "North Countrie." Robust, muscular, awkward in gait, with a somewhat gruff voice, he might have passed as a Cumberland peasant whose whole life had been lived on the fells, and in spite of his sedentary life he possessed a considerable store of physical energy, as may be gathered from the fact that even late in life he was a tireless walker amongst his native hills.

His head, as far as can be judged from statues and portraits, bears a marked resemblance in form to that of Newton. The forehead is broad, the eyebrows prominent, the eyes deep set. The close firm mouth, square chin, and rather massive jaws give to the face a distinct air of determination, with an indication of doggedness—not to say obstinacy. From behind the large spectacles which he wore, the eyes seem to look forward quietly and calmly, the whole expression being what is often called philosophic—a true outward sign of the inner nature of the man.

On leaving the college he lived for some time in a house in Faulkner Street, afterwards in the house of a Quaker, John Cockbain, and from 1805 till 1830 with the Rev. William Johns. Miss Johns tells how this last change of residence came about :—

"As my mother was standing at her parlour window, one evening towards dusk, she saw Dr. Dalton passing on the other side of the street, and on her opening the window, he crossed over and greeted her. 'Mr. Dalton,' said she, 'how is it that you so seldom come to see us?' 'Why, I don't know,' said he; 'but I have a mind to

come and live with you.' My mother thought at first that he was in jest; but finding that he really meant what he said, she asked him to call again next day, after she should have consulted my father. Accordingly he came and took possession of the only bedroom at liberty, which he continued to occupy for nearly thirty years."

His sole form of recreation seems to have been the game of bowls. He belonged to a bowling club which met every Thursday afternoon at the "Dog and Partridge," a tavern then in the country but now in Manchester. Here he played, with considerable enthusiasm, a definite number of games, took his tea, smoked his churchwarden, and came back to his laboratory to work. The reason he gave for choosing Thursday as the day of recreation was that he preferred to take his Saturday half-holiday in the middle of the week.

The fullest account of his habits and daily life is to be found in a journal in which Miss Johns has recorded her recollections, from which the following is taken:—

"On Sundays he always dressed himself with the most scrupulous attention to neatness, attended public worship twice—except when indisposed, or on very particular occasions, which, however, the writer does not remember to have occurred a dozen times in all—dined during his life with his friend Mr. Thomas Hoyle, the printer of Mayfield, and, returning home to tea, spent the evening in his philosophical pursuits.

"His dress was that usually worn by the Quakers, avoiding, however, the extreme of formality, and always of the finest texture; hat, gloves, gaiters, and even a

handsome cane to correspond. In his general intercourse, also, he never adopted their peculiar phraseology.

"With respect to his religious principles, I should be disposed to think that he had never made theology, properly so-called, a study. He certainly never mentioned having done so; but his reverence for the great Author of all things was deep and sincere, as also for the Scriptures, in which His revealed will is expressed. When the occasion called for it, I have heard him express his sense of the duty and propriety of the religious observance of Sunday, and also his serious disapprobation of its violation. Although frequently solicited, he refused all invitations to dine out on that day, except a very few times at Dr. Henry's, and once or twice elsewhere, when, as he observed to me, he was asked to meet a very distinguished professor whom he should otherwise have missed the opportunity of seeing. But when the same friend, presuming on his former compliance, again invited him on that day he received a refusal, which prevented any further application.

"His week-days—every day, and all day long, were spent in his laboratory, with the exception of Thursday afternoons, when he accompanied a party of friends about three miles into the country to bowl, and entered into the amusement with a zest infinitely amusing to all who were present. He also spent a few minutes, generally between light and dark, at the Portico, in reading the daily papers.

"He rose about eight in the morning, always lighted his laboratory fire before breakfast, after which meal he

finished his toilet and repaired to his laboratory, which he seldom left until dinner. He dined at one, but always came in in much haste when dinner was partly over—I suppose to save time. He ate moderately and drank only water. He was obliged to eat slowly, on account of the conformation of his throat, which was very narrow. After dinner he always spent about a quarter, rarely half-an-hour, in chatting with the different members of the family, or any visitor, or in looking over any chance publication lying on the table.

"After spending the afternoon in his laboratory, he drank tea at five, rarely coming in until the family had nearly finished. He was very methodical in the quantity he took at meals. After tea, to his laboratory again, where he stayed until nine (supper-time), when he regularly shut up for the night, ate a light supper, generally of meat and potatoes, until about his sixtieth year, when he changed this for meal porridge, with milk or treacle, or occasionally a couple of eggs.

"After supper we all sat together, and generally had a nice chat, for which the labours of the day had excellently prepared us all; and I will venture to say that few firesides have ever presented a scene of more innocent and pleasant recreation than did ours during these the busy years of our life. The doctor took little part in the conversation, though he showed that he listened by frequently smiling, and now and then uttering some dry, laconic witticism in reference to what was passing. He and my father smoked their pipes unremittingly. Not unfrequently we were joined by two or three political friends, who talked over the news

of the times, etc. The doctor enjoyed their society, but took little part in the conversation, in politics none whatever, nor for years had we any idea what his views on the subject were (Conservative). Occasionally he took the chief part in conversation, but this only when we were quite alone, or when Mr. Ewart stepped in, as he sometimes did. He and the doctor had a great esteem for each other, which lasted through life. When, however, this gentleman was our visitor, the evening seldom ended without my father and he getting deeply into metaphysics—a favourite study with both. The doctor generally listened intently, but from an occasional ironical smile I used to suspect that he thought it mostly 'vain wisdom all and false philosophy.'"

His holiday times were spent chiefly among his native hills of Cumberland where he had ample opportunities of indulging his taste for meteorological and scientific studies. These excursions were often made in company with old friends. Jonathan Otley, the veteran guide of Keswick who spent his life in exploring and describing the Lake District, was one of Dalton's most constant companions and has recorded some of his recollections of Dalton and the tours they made. Referring to Dalton's visits to Cumberland, Otley says:—

"Dr. Dalton usually travelled by stage as far as the coach served his purpose; the rest of the journeyings were chiefly accomplished on foot. He used to say that a little mountain exercise brought into play a certain set of muscles which would otherwise turn rigid and inactive. He was active and persevering in climbing a mountain; especially when he came in sight of the goal

there was no keeping pace with him. In descending, or on rough ground I was fully his equal; my stronger shoes enabled me to venture more freely. The barometer which he carried was of the most simple construction, yet its action was more intelligible than some fitted up in a more expensive way. His eyes, though subject to some defects, were very exact in estimating small divisions of space. His mode of calculating altitudes generally came out something higher than what has subsequently been given in the Ordnance Survey; but for his purpose the greatest exactness was not required. In later years he declined bringing his barometer, as he had the privilege of using one belonging to the Rev. Dr. Pearson, and afterwards one of my own construction. He was never adverse to taking Matthew Jopson's advice in taking a little brandy to mix with the water from Brownrigg Well, but he was very abstemious in using it. Although these excursions have been undertaken chiefly as recreations, they have not been without their use. They assisted in the investigation of the constitution of the atmosphere, and we have been enabled to make a step in advance of our predecessors in the geographical delineation of the district. Although the doctor always treated me as a companion, he would never permit me to go without some pecuniary remuneration—I must not say for loss of time, as no time could be said to be lost that was spent in his company, he was so affable and communicative.

"When on the last-mentioned occasion I would have declined what he offered, he said I must take it; it might probably be the last—and, as far as regarded

mountain excursions or journeying in company, so it was. I saw him at Keswick two or three times after that, but still with a kind of melancholy pleasure."

Even in his periods of relaxation from the arduous duties which his scientific work involved, Dalton could not refrain from exercising those powers of observation which serve to stamp him as a scientist of the first order. He speaks, in reference to these visits to the Lake Districts, of the "additional gratification" he derived from being "enabled to unite instruction and amusement." He himself tells us that he had "a portable barometer not less than seven times upon the summit" (of Helvellyn), and could "fully answer for the accuracy of the barometric variation between the Valley of Wythburn at the foot, and the summit of the mountain."

It is interesting to notice that Dalton's first impressions of London were scarcely favourable. He describes it as a most surprising place, well worth seeing once, but concludes that it would be the "most disagreeable place on earth for one of a contemplative turn to reside in."

Very different were his feelings when he returned to his native place, where he was always at home amongst the simple peasant folk who were his friends and contemporaries. We learn that he was always ready and happy to join them by the fireside and enjoy a "real gude crack" about earlier days. Even after all his triumphs and scientific achievements he was at heart the simple countryman of frugal tastes, speaking the broad dialect of the Cumberland fells, and, in his

associations with old friends, quite forgetful of the fact that he was the distinguished scientist whose name had travelled far and wide.

In habits and manners he was as simple as a man well could be. Because of his devotion to his work he was of necessity debarred from much of the ordinary social intercourse. A true Quaker, he was staid and sober in all his relations with the larger world, of which he saw but little. In answer to inquiries as to why he had never married his invariable reply was that he had never had time. In one letter he says that his "head is too full of triangles, chymical processes, and electrical experiments, etc., to think much of marriage." But in spite of these protestations there is abundant evidence to show that the sober man of science, who never had time to think much of marriage, yielded to the charms of a Quaker lady named Nancy Wilson. Miss Johns, who probably knew more of Dalton's private life than almost any one else, says that he cherished Nancy Wilson's memory throughout his life, and could never speak of her without betraying considerable emotion. As the lady was engaged at the time he made her acquaintance, it would seem that there was nothing more than a very warm friendship between them, and some explanation of Dalton's emotion may lie in the fact that she died young.

He himself confesses to an *affaire de cœur* which seems to have been something quite out of the common course of his life. In writing to his friend Robinson he gives expression to his feelings concerning this *affaire*. He says:—

"I must not, however, omit to mention that I was completely Sir Roger de Coverleyed a few weeks ago.

"The occasion was this: Being desired to call upon a widow—a Friend, who thought of entering her son at the academy—I went, and was struck with the sight of the most perfect figure that ever human eyes beheld, in a plain but neat dress; her person, her features were engaging beyond all description. Upon inquiry after, I found she was universally allowed to be the handsomest woman in Manchester. Being invited by her to tea a few days after, along with a worthy man here, a public Friend (a Quaker minister), I should have in any other circumstances been highly pleased with an elegant tea equipage, American apples of the most delicious flavour, and choice wines: but in the present these were only *secondary* objects. Deeming myself, however, full proof against *mere beauty*, and knowing that its concomitants are often ignorance and vanity, I was not under much apprehension. But she began to descant upon the excellence of an exact acquaintance with English grammar and the art of letter-writing; to compare the merits of Johnson's and Sheridan's dictionaries; to converse upon the use of dephlogisticated marine acid in bleaching; upon the effects of opium on the animal system, etc., etc. I was no longer able to hold out, but surrendered at discretion. During my *captivity*, which lasted about a week, I lost my appetite, and had other symptoms of *bondage* about me, as incoherent discourse, etc., but have now happily regained my freedom."

The reasons for this rather sudden cure of love-

sickness, if one may apply such a term to a rather hard-headed, unemotional philosopher, do not transpire. Possibly he came to regard the widow's brilliant conversation on subjects dear to himself, as part of those wiles which are commonly attributed to the gentler sex. But whatever was the cause of the disillusionment, he apparently soon forgot the subject of the above revelations, and, it seems, was attracted by the daughter of a friend of his. He writes to his brother Jonathan eulogizing this lady, whose name was Hannah, and who, from his description, seems to have satisfied particularly well the requirements of her somewhat captious Quaker friend and critic. He speaks of her in the following terms:—

" . . . I have never met with a character so finished as Hannah's. What is called strength of mind and sound judgment she possesses in a very eminent degree, with the rare coincidence of a quick apprehension and the most lively imagination. Of sensibility she has a full share, but does not affectedly show it on every trivial occasion. The sick and the poor of all descriptions are her personal care. Though undoubtedly accustomed to grave and serious reflections, all pensiveness and melancholy are banished from her presence, and nothing but cheerfulness and hilarity diffused around.

" Her uncommon natural abilities have been improved by cultivation, but art and form do not appear at all in her manner: all is free, open, and unaffected. Extremely affable to all, though everyone sees and acknowledges her superiority, no one can charge her

with pride. She is, as might be expected, well pleased with the conversation of literary and scientific people, and has herself produced some essays that would do credit to the first geniuses of the age, though they are scarcely known out of the family, so little is her vanity.

"Her person is agreeable, active, and lively. She supports conversation whether serious, argumentative, or jocular with uncommon address. In short, the *tout ensemble* is the most complete I ever beheld."

The letter concludes with a comparison of Hannah with her sister Ann who seems to have been but little inferior in good qualities to the sister who found the greater favour in Dalton's eyes. Thoroughly characteristic of John Dalton is the following sentence, which appears at the end of the above-mentioned comparison:—

"I dwell with pleasure upon the character of these two amiable creatures, but would not have thee communicate my sentiments to others."

However, in spite of the charms of these ladies of his acquaintance, Dalton never found "time to marry," but continued in his old ways of scientific work. These ways, as has been mentioned before, were of the most regular and orderly nature. His devotion to work debarred him from enjoying many of the social pleasures which otherwise might have softened to some extent the harder features of his character.

His tastes were simple, his habits frugal, and he never departed from the regular mode of life which characterized his early years.

It was perhaps largely on account of his temperate and well-ordered manner of life that he was able to

accomplish so much, but in addition to this there was always present that enthusiasm and love of knowledge, the fruit of which is to be found in the researches by which his name is best known, that is to say in the work which gave rise to the hypothesis known to-day as Dalton's Atomic Theory.

CHAPTER VI

THE ATOMIC THEORY

In the history of civilization we find in its earliest chapters speculation as to matter and its nature. So soon as man raised himself above that level when the only demands to be satisfied were those of the body, so soon as there came the first step in advance from the unreasoning to the reasoning state, he turned his attention to himself, to his surroundings, to everything he could see and touch, in other words to what we know by the term matter. If we leave out of account the metaphysical and philosophical discussions which have raged round the question of its existence, it is the problem of the nature of matter with which the natural philosopher has been concerned during the whole period of man's intellectual development.

It is in the writings of the early Greek schools that we find the clearest account of the views on matter which philosophers held, but there are also indications of a similar mental progress in the ancient writings of Egypt and India. Two views were held, one that matter was continuous and capable of an infinite amount of division, the other that it was made up of a vast number of discrete particles, infinitesimal in size and unchanging in nature. Adherents to the former way of thinking have not been wanting, but it is to the latter view that

most attention has been given, and it is this conception of a discontinuous structure which has proved most useful in scientific thought and investigation. Thales held that water was the basis of all things, his arguments being founded upon the fact that without water there was no life, either animal or vegetable. The solid earth seemed to spring from water, and water passed into the air and disappeared, so that water must be the fundamental principle underlying all life and matter.

At a later date came the view that air was the parent substance, capable of assuming qualities which accorded with the conceptions of the "elements" fire, air, earth and water. This idea was probably first put forward by Anaximenes and afterwards more fully expounded by Diogenes, who seems to have regarded this air as producing earth and water by precipitation or condensation, though it is hard to see in what respects the principle, air, from which all matter is made, differs from the air as revealed to the senses.

Anaxagoras, amongst the early thinkers, approached most nearly to the "discontinuous" theory of matter, for he looked upon all elements (using the word in its original sense) as being made up of infinitesimal particles; and to the presence of an excess of one kind of particle over another kind were due the distinctive properties of the different forms of matter.

Zeno and Empedocles both expressed the idea that matter was composed of four elements, and the latter taught that these elements were made up of minute particles, which were capable of division and yet remained united in their unchanging form.

It is however when we consider the philosophy of Leucippus and Democritus that we first find anything that really approximates to the present concept of an atomic structure of matter. The atoms are indivisible particles which by virtue of their arrangement give to matter its varying properties. Possibly here we have a faint foreshadowing, a prophetic glimpse of the wide-reaching kinetic theory of matter which revolutionized scientific thought, for it would appear that the atoms of Democritus possessed the power of motion.

The views of Leucippus, which were adopted and elaborated by the Epicurean school, are recorded with further poetic embellishments in Lucretius' poem *De rerum naturâ*. The argument is briefly as follows. The formation of visible or tangible bodies from discrete particles is to be looked upon as a process of reproduction, and this process — here lies a purely gratuitous assumption—is slower than that of disintegration. As a consequence of this, the disintegration must end at some point. The end is marked by the formation of the atom, a finite, invisible, discrete particle. Matter was therefore regarded as made up of an infinitely large number of atoms of varying sizes and shapes which possessed the power of motion.

"Matter mingled and massed into indissoluble union
Does not exist For we see how wastes each separate substance.
So flow piecemeal away, with the length'ning centuries, all things,
Till from our eye by degrees that old self passes, and is not.
Still Universal Nature abides unchanged as aforetime,

> Whereof this is the cause. When the atoms part from a substance,
> That suffers loss; but another is elsewhere gaining an increase:
> So that as one thing wanes, still a second bursts into blossom,
> Soon, in its turn, to be left. Thus draws this universe always
> Gain out of loss."

The conception of Lucretius is perhaps as clear, though wanting in mathematical conciseness, as any expressed between his times and those of Dalton; for, during the centuries in which chemical science was represented by alchemy, we find but little logical reasoning which is not overburdened with fanciful and mystic writings. The elements of the alchemists were still fire, earth, air, and water, capable of transformation, the one into the other, and so differing radically from the true idea of an element. This idea is clearly to be seen in the writings of Albertus Magnus in which the birth of one element is stated to be the result of the corruption or death of another. The existence of these elements was in some mysterious manner dependent upon something abstract, a *prima materia*. This essence of matter was called *yle* by Roger Bacon, who flourished about the same time as Albertus Magnus. His views are clearly expressed in the following extract from his *De Arte Chymiæ*:—

"Elementa sunt quatuor, ignis, aqua, aer, terra, modi, id est proprietates, sunt quatuor, calor, frigiditas, siccitas et humiditas; et yle est res in qua non est calor, nec frigiditas, nec siccitas, nec humiditas, et non est corpus. Et elementa sunt facta de yle; et unumquodque elementorum convertitur in naturam alterius elementi et omnis res in quamlibet."

Geber taught that the metals were transmuted one into the other by nature but denied to man the possibility of accomplishing the same change.

It was, then, in this conception of a passage from one "element" to another that the alchemists differed from the early atomic philosophers, and throughout the long period during which alchemy held sway, there was no advance made towards a simple explanation of the structure of matter but rather a falling away from those views of Lucretius which to-day we regard as comparatively close guesses at the truth of this fundamental question.

It is beyond the scope of the present book to treat in detail of the development of these theories of matter and its structure, but it is necessary to say a few words concerning the progress made from the time when alchemy began to wane to the time when the atomic theory of Dalton, supported by experimental evidence, placed the subject upon a firm foundation.

Boyle, in his "Sceptical Chymist," makes use of the following words:—

"Now, if it be true, as 'tis probable, that compound bodies differ from one another, in nothing but the various textures, resulting from the magnitude, shape, motion, and arrangement of their small parts, it will not be irrational to conceive that one and the same particle of universal matter, may by various alterations and contextures be brought to deserve the name sometimes of a sulphureous, and sometimes of a terrestrial or aqueous body."

Here again we find a definite conception of atoms

and a distinct leaning to an atomic view of matter, for later in the book Boyle condemns the ordinary view of the four elements of all previous writers. His ideas as to the atoms seem to be similar to those of Newton who says: "It seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures and with such other properties, and in such proportion to space, as most conduced to the end for which He formed them."

But even these clear ideas were again confused by the theory of Phlogiston introduced by Becher and Stahl in the eighteenth century.

Towards the end of the eighteenth century, however, more definite views were put forward, and we find that so early as 1786 Bryan Higgins expressed the opinion that "elastic fluids unite with each other in limited proportions only; and that this depends upon the combinations of their particles or atoms with the matter of fire, which surrounds them as an atmosphere, and makes them repulsive of each other."

He further distinguishes "between simple elastic fluids, as composed of particles of the same kind, and compound elastic fluids, as consisting of two or more particles, in what he calls molecules, definite in quantities themselves, and surrounded by definite proportions of heat."

William Higgins, a pupil of Dr. Bryan Higgins, published a work entitled "A Comparative View of the Phlogistic and Antiphlogistic Theories with Inductions." In view of the fact that attempts have been made to

give Higgins the credit for enunciating the atomic hypothesis, a few extracts from this work, quoted by Dr. Henry, will not be out of place.

In the above-mentioned work we find the following statements: "100 grains of sulphur, making an allowance for water, require 100 or 102 of the real gravitating matter of oxygen to form sulphurous acid gas, and as this gas is little short of double the specific gravity of oxygen, we may conclude that the ultimate particles of sulphur and oxygen contain equal quantities of solid matter, for oxygen suffers no considerable contraction by uniting to sulphur in the proportion merely necessary for the formation of sulphurous acid. Hence we may conclude that in sulphurous acid a single ultimate particle of sulphur is intimately united to a single particle of oxygen, and that, in sulphuric acid, every single particle of sulphur is united with two of oxygen, being the quantity necessary to saturation."

Here, almost for the first time, we have introduced a definite numerical conception of the behaviour of atoms, and this conception is still further explained in his remarks on water and the oxygen compounds of nitrogen. He writes:—

"As two cubic inches of hydrogen require but one of oxygen to condense them, we must suppose that they contain an equal number of divisions [atoms], and that the difference of their specific gravity depends chiefly on the size of their ultimate particles, or we must suppose that the ultimate particles of hydrogen require two or three or more to saturate them. If this latter were the case, we might produce water in an intermediate

state, as well as sulphuric or nitrous acids, which appear to be impossible; for in whatever proportion we mix our airs, or under whatsoever circumstances we combine them, the result is invariably the same. This likewise may be observed with respect to the decomposition of water. Hence we may justly conclude that water is composed of molecules, formed by the union of a single particle of oxygen to an ultimate particle of hydrogen, and that they are incapable of uniting to a third particle of either of their constituent principles."

It is quite easy to see the fallacy in this method of reasoning, but one cannot refrain from admiring the clear view Higgins possessed at a time when all phenomena were surrounded by a cloud of speculation which instead of helping towards a more perfect understanding of their nature, succeeded in rendering it more obscure.

Higgins, in 1814, published another book written solely with the idea of vindicating his claims to priority in the enunciation of the atomic theory. In this work he practically accuses Dalton of plagiarism for he says :—

"I cannot with propriety or delicacy say that Mr. Dalton is a plagiarist, although appearances are against him. Probably he never read my book; yet it appears extraordinary that a person of Mr. Dalton's industry and learning should neglect one of the few works that were expressly written on the subject of the theory. At the time it was published, there were one thousand copies of it sold."

Dr. Henry considers this charge of plagiarism to be entirely unfounded, and quotes the testimony of various

witnesses in support of his contention. He states that he heard his father assert, on more than one occasion, that Dalton knew nothing of the work of Higgins until some years after the publication of the *New System of Chemical Philosophy*.

Higgins must undoubtedly be counted as a pioneer in this work, but the credit of being the founder of the atomic theory as it is known to-day does not belong to him. As Angus Smith says of him, "He had seen the right road, but dared not go farther. But we must take his own apology, 'Est quoddam prodire tenus, si non datur ultra.'"

It seems clear that the claims to priority of discovery of the atomic theory which have been put forward on behalf of Higgins are not upheld, but that it was left to Dalton to formulate clearly for the first time the new view that compounds are made up of atoms which combine together in definite ratios.

At the close of the eighteenth century, Richter in Germany was engaged in the investigation of the laws that govern the neutralization of acid by bases. The credit for the discovery of the law underlying this phenomenon has been wrongly given to Wenzel. It was Richter in his work entitled *Ueber die neuren Gegenstände der Chemie* who first showed that the weights of two acids neutralized by the same quantity of a base *a*, require for neutralization the same amount of another base *b*. In this work, Richter states that he regards every compound as made up of particles of exactly the same nature and composition as the whole, and that the affinity which causes an acid to neutralize

or combine with a base is present in every particle. He performed a vast number of experiments and prepared many tables of results. These latter were combined by Fischer into one table which forms the first real list of equivalents, the full significance of which was not perceived until after the enunciation by Dalton of his atomic theory.

Simultaneously with the growth of the atomic conception there was developed the theory of the equivalent. Richter's book was published in 1795, long before Dalton expressed his views on atomic combination, and though Dalton undoubtedly knew of the results of Richter's work on reciprocal proportions, he nowhere acknowledged that he had received assistance from them. Angus Smith seems to think that Dalton was only acquainted with an outline of these results, and that though they may have to some extent corroborated his own conclusions, they played no part in the establishment of those fundamental ideas which have exercised so profound an influence upon the subsequent development of chemistry.

We are now in a position to consider the work of Dalton in connection with the atomic view of matter, the various conceptions of which have been traced in the preceding pages. To him is due the credit of having enunciated a theory which explained the laws found to govern the formation of compounds. No claim is made, nor did he ever urge one, that he introduced the atomic conception. In his *New System of Chemical Philosophy*, he speaks of his views in the following terms:—

"A very familiar instance is exhibited to us in water, of a body, which, in certain circumstances, is capable of assuming all the three states. In steam we recognise a perfectly elastic fluid, in water a perfect liquid, and in ice a complete solid. These observations have tacitly led to the conclusion, which seems universally adopted, that all bodies of sensible magnitude, whether liquid or solid, are constituted of a vast number of extremely small particles, or atoms of matter bound together by a force of attraction, which is more or less powerful according to circumstances, and which, as it endeavours to prevent their separation, is very properly called, in that view, *attraction of cohesion;* but as it collects them from a dispersed state (as from steam into water) it is called *attraction of aggregation*, or more simply, *affinity*.[1]

We see that Dalton possessed a clearer and more definite view of these atoms than any previous thinker on the subject. He advanced further than either Higgins or Richter in this respect—he regarded the combinations of atoms as taking place according to definite rules of number, as well as by the rules of number and weight. Richter, it is true, quotes the words, "God ordered all things by measure, number, and weight," but he was not sufficiently clear-sighted to see how they might be applied to the results of his own careful work. Dalton however saw into the heart of the problem and showed by experiment that atoms do combine together according to definite rules of number. The elaboration of this view and the collection of experi-

[1] *A New System of Chemical Philosophy*, Manchester, 1808, pp. 141-142.

mental evidence in support of it form the most important part of Dalton's scientific work and constitute his greatest claim to remembrance by those whose scientific labours are based upon this most important theory.

CHAPTER VII

THE ORIGIN OF DALTON'S ATOMIC THEORY, AND ITS DEVELOPMENT

FROM the study of Dalton's earlier scientific work, we learn that he was in the habit of regarding "elastic fluids" as being made up of *particles*. We find that he regarded the atmosphere and gases generally as composed of "particles . . . arranged in horizontal strata like a pile of shot."

The question as to how Dalton came to these views on gases and the formation of compounds from their constituent atoms, has been the occasion for much discussion. Hitherto, it was regarded as established that Dalton first discovered the law which is now known under the name of the law of combination in multiple proportions and then sought for an explanation of the fact. According to this view, the atomic theory was based upon experiment and was the direct result of his observations. More recently, however, various manuscript notes for lectures and laboratory records made by Dalton have been discovered amongst the archives of the Literary and Philosophical Society of Manchester. The most important of these have been most carefully studied and the results embodied in a work published in 1896.[1] From these comparatively recent discoveries we

[1] *A New View of the Origin of Dalton's Atomic Theory*, by Henry E. Roscoe and Arthur Harden. London, Macmillan & Co., 1896.

are compelled to adopt the view that the earlier speculations as to the genesis of the theory are entirely wrong, being, in fact, the inverse of what appears to be the true explanation of the case. This rather contradictory state of affairs calls for some explanation, and this seems to be afforded by the circumstance that in none of the previously published works of Dalton was there any clear account of the train of thought which led him to enunciate this great principle, and all the evidence at our disposal was that derived from the remarks of various contemporaries with whom Dalton had discussed the subject. One weighty piece of evidence is a statement by Dr. Thomson, who visited Dalton in 1804 and had a conversation with him about his theory. He says :—

"Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas, at that time imperfectly understood, and the constitution of which was first fully developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them that the constituents of both were carbon and hydrogen and nothing else; he found, further, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas a compound of one atom of carbon and one atom of hydrogen; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to carbonic acid, water, ammonia, etc., and numbers representing the atomic weights of

oxygen, azote, etc., deduced from the best analytical experiments which chemistry then possessed." [1]

Later in speaking of Wollaston and his work, Thomson says :—" Mr. Dalton founded his theory on the analysis of two gases, namely, protoxide and deutoxide of azote."

In his *System of Chemistry*, Thomson speaks of his interview with Dalton and warns the reader "not to decide upon the notions of that philosopher (Dalton) from the sketch which I have given, derived from a few minutes' conversation, and from a small written memorandum," so that too great reliance cannot be placed upon evidence deduced from Thomson's exposition of the origin of the atomic theory.

Further, in speaking of carburetted hydrogen or marsh gas, Dalton says :—" No correct notion of the constitution of the gas about to be described seems to have been formed till the atomic theory was introduced and applied in the investigation." [2] Debus [3] points out in this connection that the real significance of the passage just quoted lies in the fact that the atomic theory helped to solve the problem of the constitution of marsh gas and that the converse does not hold good though maintained by previous writers.

It therefore seems probable that Dalton retained no very clear idea of the sequence of thought which had led him to enunciate the theory under discussion, and that, as Henry in his life of Dalton suggests, the state-

[1] Thomson's *History of Chemistry*, Vol. II. p. 291.

[2] *New System*, Vol. I. p. 444.

[3] *Ueber einige Fundamental sätze der Chemie.* Cassel. Hofbuchhandlung von Gustav Klaunig, 1894.

ment of Dr. Thomson recorded in 1804, while the matter was under close consideration, is far more likely to be accurate than those made by Dalton later in life to other chemists.

Dr. Henry quotes the following account of a conversation which his father had with Dalton :—

"At page 132, *et seq.* of that volume [the work on Meteorology] gives distinct anticipations of his views on the separate existence of aqueous vapour from atmospheric air. At that time the theory of chemical solution was almost universally received. *These views were the first germs of his atomic theory, because he was necessarily led to consider the gases as constituted of independent atoms.* Confirmed the account he before gave me of the origin of his speculations leading to the doctrine of simple multiples, and of the influence of Richter's table in exciting these views."

Further, Dr. Henry gives the following extract from his own journal :—

"1824, February 5. The speculations which gave birth to the atomic theory were first suggested to Mr. Dalton by the experiments of Richter on the neutral salts. That chemist ascertained the quantity of any base, as potash for example, which was required to saturate 100 measures of sulphuric acid. He then determined the quantities of the different acids which were adequate to the saturation of the same quantity of potash. The weights of the other alkaline bases entering into chemical combination with 100 parts of sulphuric acid were then obtained; and these it is obvious (?) would be equivalent to the saturation of the

quantities of the different acids before determined. On these principles a table[1] was formed, exhibiting the proportions of the acids and the alkaline bases constituting neutral salts. It immediately struck Mr. Dalton that *if these saline compounds were constituted of an atom of acid and one of alkali, the tabular numbers would express the relative weights of the ultimate atoms.* These views were confirmed and extended by a new discovery of Proust. He maintained that the compounds of iron and oxygen are strictly definite; in other words, that 100 parts of iron combine either with twenty-eight or forty-two parts of oxygen, but with no intermediate quantity. He did not, however, discover the existence of multiple proportions."

These passages show clearly that Dalton's views were undoubtedly influenced by the work of Richter but they fail to furnish completely the sequence of thought which led to the actual enunciation of the atomic hypothesis.

Much light has been thrown upon this vexed question by the publication of Dalton's lecture notes and the clearest and most satisfactory solution of the difficulty is to be derived from a consideration of these. An admirable statement of the case is to be found in the work to which reference has already been made, *A New View of the Origin of Dalton's Atomic Theory*, and in order to obtain an idea as to how the atomic theory arose, quotations from these recently discovered papers will be freely made.

In 1810 Dalton delivered a course of twenty lectures at the Royal Institution in London, and in one of his

[1] Probably that published by Richter in 1803.

note-books is found material for these lectures, some of which is of the first importance for the purpose we have under consideration. The seventeenth lecture, given on January 27th, 1810, is of this nature and it will be as well to give this in full.

"*Lecture* 17.—CHEMICAL ELEMENTS.

"As the ensuing lectures on the subject of *chemical elements* and their combinations will perhaps be thought by many to possess a good deal of novelty, as well as importance, it may be proper to give a brief historical sketch of the train of thought and experience which led me to the conclusions about to be detailed.

"Having been long accustomed to make meteorological observations, and to speculate upon the nature and constitution of the atmosphere, it often struck me with wonder how a *compound* atmosphere, or a mixture of two or more elastic fluids, should constitute apparently a homogeneous mass, or one in all mechanical relations agreeing with a simple atmosphere.

"Newton had demonstrated clearly, in the 23rd Prop. of Book 2 of the *Principia*, that an elastic fluid is constituted of small particles or atoms of matter, which repel each other by a force increasing in proportion as their distance diminishes. But modern discoveries having ascertained that the atmosphere contains three or more elastic fluids, of different specific gravities, it did not appear to me how this proposition of Newton would apply to a case of which he, of course, could have no idea.

"The same difficulty occurred to Dr. Priestley, who discovered this compound nature of the atmosphere. He could not conceive why the oxygen gas, being specifically heaviest, should not form a distinct *stratum* of air at the bottom of the atmosphere, and the azotic gas one at the top of the atmosphere. Some chemists upon the Continent, I believe the French, found a solution of this difficulty (as they apprehended). It was *chemical affinity*. One species of gas was held in solution by the other; and this compound in its turn dissolved water; hence *evaporation*, *rain*, etc. This opinion of air dissolving water had long before been the prevailing one and naturally paved the way for the reception of that which followed, of one kind of air dissolving another. It was objected that there were no decisive *marks* of chemical union, when one kind of air was mixed with another—the answer was, that the affinity was of a very *slight* kind, not of that energetic cast that is observable in most other cases.

"I may add, by the bye, that this is now, or has been till lately, I believe, the prevailing doctrine in most of the chemical schools of Europe.

"In order to reconcile or rather adapt this chemical theory of the atmosphere to the Newtonian doctrine of repulsive atoms or particles, I set to work to combine my atoms upon paper. I took an atom of water, another of oxygen, and another of azote, brought them together, and threw round them an atmosphere of heat, as per diagram; I repeated the operation, but soon found that the watery particles were exhausted (for they make but a small part of the atmosphere). I next

combined my atoms of oxygen and azote, one to one; but I found in time my oxygen failed; I then threw all the remaining particles of azote into the mixture, and began to consider how the general equilibrium was to be obtained.

"My triple compounds of *water*, *oxygen*, and *azote* were wonderfully inclined, by their superior gravity, to descend and take the lowest place; the double compounds of *oxygen* and *azote* affected to take a middle station; and the azote was inclined to swim at the top. I remedied this defect by lengthening the wings of my heavy particles, that is, by throwing more heat around them, by means of which I could make them float in any part of the vessel; but this change unfortunately made the whole mixture of the same specific gravity as azotic gas—this circumstance could not for a moment be tolerated. In short, I was obliged to abandon the hypothesis of the chemical constitution of the atmosphere altogether, as irreconcilable to the phenomena.

"There was but one alternative left, namely, to surround every individual particle of *water*, of *oxygen*, and of *azote*, with heat, and to make them respectively centres of repulsion, the same in a *mixed* state as in a *simple* state. This hypothesis was equally pressed with difficulties; for, still my oxygen would take the lowest place, my azote the next, and my steam would swim upon the top.

"In 1801 I hit upon an hypothesis which completely obviated these difficulties.

"According to this, we were to suppose that the

atoms of one kind did *not* repel the atoms of another kind, but only those of their own kind. This hypothesis most effectually provided for the diffusion of any one gas through another, whatever might be their specific gravities, and perfectly reconciled any mixture of gases to the Newtonian theorem. Every atom of both or all the gases in the mixture was the centre of repulsion to the proximate particles of its own kind, disregarding those of the other kind. All the gases united their efforts in counteracting the pressure of the atmosphere, or any other pressure that might be opposed to them.

"This hypothesis, however beautiful might be its application, had some improbable features.

"We were to suppose as many distinct *kinds* of repulsive powers, as of gases; and, moreover, to suppose that *heat* was not the repulsive power in any one case; positions certainly not very probable. Besides, I found from a train of experiments, which have been published in the *Manchester Memoirs*, that the diffusion of gases through each other was a *slow* process, and appeared to be a work of considerable effort.

"Upon reconsidering the subject, it occurred to me that I had never contemplated the effect of *difference of size* in the particles of elastic fluids. By *size* I mean the hard particle at the centre and the atmosphere of heat taken together. If, for instance, there be not exactly the same *number* of atoms of oxygen in a given volume of air, as of azote in the same volume, then the *sizes* of the particles of oxygen must be different from

those of azote. And if the *sizes* be different, then on the supposition that the repulsive power is heat, no equilibrium can be established by particles of unequal sizes pressing against each other.[1]

"This idea occurred to me in 1805. I soon found that the *sizes* of the particles of elastic fluids *must* be different. For a measure of azotic gas and one of oxygen, if chemically united, would make nearly *two* measures of nitrous gas, and those *two* could not have *more* atoms of nitrous gas than the *one* measure had of azote or oxygen.

"Hence the suggestion that all gases of different kinds have a difference in the *size* of their atoms; and thus we arrive at the reason for that diffusion of every gas through every other gas, without calling in any other repulsive power than the well-known one of *heat*.

"This then is the present view which I have of the constitution of a mixture of elastic fluids.

"The different *sizes* of the particles of elastic fluids under like circumstances of temperature and pressure being once established, it became an object to determine the relative *sizes* and *weights* together with the relative *number* of atoms in a given volume. This led the way to the combinations of gases, and to the *number* of atoms entering into such combinations, the particulars of which will be detailed more at large in the sequel. Other bodies, besides elastic fluids, namely liquids and solids, were subject to investigation, in

[1] Reference is here made to a diagram which is not reproduced in the notes.

consequence of their combining with elastic fluids. Thus a train of investigation was laid for determining the *number* and *weight* of all chemical elementary principles which enter into any sort of combination one with another.

"1. Divisibility of matter considered. Atoms—*see* Newton's ideas.

"2. Elastic fluids exhibit matter in extreme division. Newton B. 2; Prop. 23. *See* diagram. Hydrogen and oxygen cannot be broken down into finer kinds by electricity. Like flour, etc., sugar, etc.

"Compound gases, as nitrous, carbonic acid, are separated into their ulterior elements by electricity . . . . *see* diagram atmosphere.

"3. Other bodies constituted of atoms as well as elastic fluids—charcoal, sulphur, phosphorus. Metals by combining with atoms of elastic fluids show that they have atoms.

"4. All atoms *of the same kind* alike in wt. bulk.

"5. Atoms of different kinds unequal in wt., etc., *see* Newton, 2.

"6. Bodies deemed simple till they are decomposed.

"7. Chemical synthesis. Exhibit two particles. *See* also Newton, 3.

"8. Table of arbitrary marks.
Gay-Lussac's notion."

In reading the preceding account it must be remembered that the *atom* of Dalton was practically the same as the *atom* of Newton; that is to say it con-

consisted of a hard impenetrable core surrounded by an atmosphere of heat which, relatively to the size of the nucleus, was very great. In the diagrams given in the *New System* this atmosphere of heat is represented by lines radiating from the centre. According to Dalton's earlier views, for all elastic fluids the ultimate particles, that is to say the nucleus with its envelope of heat, were of the same size. This however he modified later and states quite clearly his new conceptions in the following terms:—

"That every species of pure elastic fluid has its particles globular and all of a size; but that no two species agree in the size of their particles, the pressure and temperature being the same."

Doubts occurred to him as to whether heat could be regarded as a sufficient cause for repulsion, since it would seem that if heat alone were the cause, there should be no reason why a particle of oxygen should not repel one of hydrogen to the same degree as it would repel another particle of oxygen. These doubts he overcame by supposing that in a pure gas the globular particles were symmetrically arranged in horizontal strata, so that the pressure was uniform throughout. In the case of a mixture of gases, this state of things could not exist, for the particles being of different sizes would not show the same symmetrical arrangement, their points of contact would not be the same and in consequence there would be continual movement in the attempt to establish equilibrium.

In the *New System* plates are to be found which help

to make this point clear. Three gases are chosen, "simple and compound but not mixed." These are hydrogen, nitrous gas, and carbonic acid gas, and in each case the arrangement of the particles is symmetrical. In the case of the two compound gases, the "particles" consist of two and of three parts respectively, but regarded as a whole, these particles are arranged in a manner quite analogous to that in which the hydrogen particles are arranged. In the next figures are shown four particles of azote with the central nuclei and the heat envelope represented by radiating lines. These radiating lines meet and are arranged quite regularly so that the particles apply themselves "one to the other with facility." Touching these particles of azote are two particles of hydrogen drawn "in due proportion." The rays of these do not meet those of the azote in "like circumstance; hence the cause of the intestine motion which takes place on the mixture of elastic fluids, till the exterior particles come to press on something solid."

In the manuscript note-books, of which mention has already been made, are found entries which set at rest for ever the vexed question as to the manner in which Dalton arrived at his atomic theory. His attention was drawn to the necessity of determining the size and weight of the ultimate particles of which, according to his views, matter was composed, and under the date 6th September 1803 we find certain "Observations on the ultimate particles of bodies and their Combinations."

The table of symbols or *characters* as Dalton calls

them, which is given at the head of these observations, is as follows :—

○ Hydrogen.
⊙ Oxygen.
⦶ Azote.
● Carbone, pure charcoal.
⊕ Sulphur.

A little further on in the same note-book under the same heading occurs the following list of atomic weights. On examination of this list it will be clearly seen that the law of combination in multiple proportions is fully recognized. The numbers are derived from various analyses of water, carbonic acid, ammonia and sulphuric acid, though none of these analyses seem to have been made by Dalton himself.

"Ult. at.	Hydrogen	1
,,	Oxygen	5·66
,,	Azot	4
,,	Carbon (charcoal)	4·5
,,	Water	6·66
,,	Ammonia	5
,,	Nitrous gas	9·66
,,	Nitrous oxide	13·66
,,	Nitric acid	15.32
,,	Sulphur	17
,,	Sulphureous acid	22·66
,,	Sulphuric acid	28·32
,,	Carbonic acid	15·8
,,	Oxide of carbone	10·2"

In determining these values Dalton has, it is clear,

followed the rules which he laid down for the characterization of compounds.[1]

"ON CHEMICAL SYNTHESIS.

"When any body exists in the elastic state, its ultimate particles are separated from each other to a much greater distance than in any other state; each particle occupies the centre of a comparatively large sphere, and supports its dignity by keeping all the rest, which by their gravity or otherwise are disposed to encroach upon it, at a respectful distance. When we attempt to conceive the *number* of particles in an atmosphere, it is somewhat like attempting to conceive the number of stars in the universe; we are confounded with the thought. But if we limit the subject by taking a given volume of any gas, we seem persuaded that, let the divisions be ever so minute, the number of particles must be finite; just as in a given space of the universe, the number of stars and planets cannot be infinite.

"Chemical analysis and synthesis go no farther than to the separation of particles one from another, and to their reunion. No new creation or destruction of matter is within the reach of chemical agency. We might as well try to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce, consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.

"In all chemical investigations it has justly been con-

[1] *A New System of Chemical Philosophy*, Manchester, 1808. Part I. Chap. III. p. 211 *et seq.*

sidered an important object to ascertain the relative *weights* of the simples which constitute a compound. But unfortunately the enquiry has terminated here; whereas from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various other compounds would appear, in order to assist and to guide future investigation, and to correct their results. Now it is one great object of this work, to show the importance and advantage of ascertaining *the relative weights of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particle, and the number of less compound particles which enter into the formation of one more compound particle.*

"If there are two bodies, A and B, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple: namely,

1 atom of A + 1 atom of B = 1 atom of C, binary.
1 atom of A + 2 atoms of B = 1 atom of D, ternary.
2 atoms of A + 1 atom of B = 1 atom of D, ternary.
1 atom of A + 3 atoms of B = 1 atom of F, quaternary.
3 atoms of A + 1 atom of B = 1 atom of G, quaternary, etc., etc.

"The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.

"1st. When only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary.

"2nd. When two combinations are observed, they must be presumed to be a *binary* and a *ternary*.

"3rd. When three combinations are obtained, we may expect one to be a *binary* and the other two *ternary*.

"4th. When four combinations are observed we should expect one *binary*, two *ternary*, and one *quaternary*, etc."

The composition of the various compounds is expressed by the use of the symbols previously given, and the illustrations serve to show that the specifically lightest compound had assigned to it the simplest possible structure.

"  Nitrous oxide.
 Nitrous gas.
 Nitric acid.  Nitrous acid.
 Water.
 Ammoniac.
 Gaseous oxide of carbon.
 Carbonic acid.
 Alcohol? ether?
 Sulphureous acid.
 Sulphuric acid."

It is interesting to notice that the symbols for hydrogen and oxygen were interchanged later, for in the *New System*, Part I. page 219, we find what is practically an expansion of the above table, wherein the character for hydrogen is written and that for oxygen

As we have already seen, Dalton considered the determination of the relative weights of the atoms as a

matter of the greatest importance. In his manuscript note-books are to be found details of these determinations. A full account of these is contained in *A New View of Dalton's Atomic Theory*, and the following particulars are taken from that work.

At first, Dalton directed his attentions to the determination of the atomic weights of the non-metals, and in the following table [1] a list of these is given.

TABLE A

	(1) 1805	(2) 1803	(3) 1803	(4) 1805	(5) 1807	(6) 1806	(7) 1806	(8) 1808	(9) 1810	(10)	(11)
Hydrogen .	1	1	1	1	1	1	1	1	1	1·14	1·008
Oxygen .	5·66	5·66	5·5	5·5	6	7	7	7	7	16	16
Azote . .	4	4	4	4·2	5	5	5	5	5	11·4 15	14
Carbon .	4·5	4·4	...	4·3	...	5	5	5	5·4	12·3	12
Sulphur .	17	14·4	...	14·4	...	22	12	13	13	29·7	32
Phosphorus	...	7·2	...	7·2	...	9+	9·3	9	9	25 7 27	31

(1) Note-book, i. 248, 6th September 1803.
(2) Note-book, i. 258, 19th September 1803.
(3) Note-book, i. 260, September 1803.
(4) *Manchester Memoirs* (2), i. 287 (1805).
(5) Thomson's list (1807), probably given to Thomson by Dalton in 1804, or perhaps later.
(6) Note-book, ii. 282, 23rd August 1806; and ii. 284, 14th August 1806.
(7) Note-book, ii. 247, 16th September 1806; and ii. 256, 22nd October 1806.
(8) *New System*, part i., p. 219.
(9) *New System*, part ii., p. 352.
(10) The numbers of (9), calculated to O=16, and assuming the modern formulæ. Two numbers are given for Nitrogen and Phosphorus, one calculated from Dalton's formula of the Hydride, the other from that of the Oxide.
(11) The modern numbers, O=16.

[1] *A New View*, p. 83.

The publication of Dalton's note-books has enabled us to find out how he was guided in assigning particular values to the different elements. Throughout his work he adopted unity as the atomic weight of hydrogen; that of oxygen was fixed from the analysis of water, assuming the composition to be one atom of oxygen and one atom of hydrogen. The value 5·66 is obtained from Lavoisier's analysis of water. According to this water is made up of 85 of oxygen and 15 of hydrogen. The later number 7 was adopted by Dalton as a result of Gay-Lussac and Humboldt's work in 1805.

The number 4 for nitrogen is based upon an analysis of ammonia by Austin (1788). Berthollet's analysis of ammonia led to the value 4·2. Davy's analysis of the oxides of nitrogen gave $N=5{\cdot}6$ $(O=7)$, while that of ammonia gave $N=4{\cdot}7$. Dalton regarded ammonia as N H and thought 5·1 was the best value for nitrogen as determined from the oxides.

The atomic weight of carbon was derived from Lavoisier's analysis of carbonic acid gas:—

72 oxygen
28 carbon

100

With $O=5{\cdot}66$, carbon is 4·4, $O=5{\cdot}5$, carbon is 4·3, $O=7$, carbon is 5·4. Dalton probably chose 5 as the nearest whole number.

In comparing the different values given to the atomic weight of sulphur, considerable differences are noticed.

This is due to the uncertainty which existed as to the composition of sulphuric acid. The value 17 is based upon Chenevix's analysis :—

$61\frac{1}{2}$	sulphur
$38\frac{1}{2}$	oxygen
100	

The numbers in columns (2) and (4) of the table are based upon Thenard's analysis :—

56	sulphur
44	oxygen
100	

Many other values are indicated in the note-books, and Dalton probably compromised, using various analyses and adopting those which seemed nearest the mean.

The values for phosphorus are based upon Lavoisier's analyses of phosphoric acid, and upon an early determination of the density of phosphuretted hydrogen.

In determining the atomic weights of the alkalis Dalton apparently took the results of analyses and calculated how much base combined with one *atomic weight* of acid. In each case the nearest whole number was employed.

The atomic weights of the metals were also determined by Dalton. In so doing he adhered to the rule that the oxide consisted of one atom of oxygen combined with one atom of metal.

In note-book i. p. 318, the following table is found.[1]

[1] *A New View*, p. 97.

It shows that Dalton adopted the rule above given to establish the composition of one oxide and calculated the value of the second.

"THEORY OF OXIDES OF METALS:—

1st oxide is 1 metal and 1 oxygen.
2nd oxide is 1 metal and 2 oxygen.

	EXPT. (1)	EXPT. (2)	THEORY. (1)	THEORY. (2)
5. Copper, .	88½ to 11½	80 to 20	88·9 to 11·1	80 to 20
6. Iron, .	73 to 27	52 to 48	68·5 to 31·5	52 to 48
13. Manganese,	80 to 20 and 60 to 40	74 to 26	80 to 20	66·7 to 33·3 and 57 to 43."

From these extracts then, it is obvious that the law of multiple proportions was established quite independently of the analyses of carburetted hydrogen, which were not made until the year following that in which the above notes were written.

Dalton performed many experiments with a view to ascertaining the composition of nitrous gas and its behaviour towards air, and apparently came to the conclusion that the conditions under which the experiments were performed had an important influence on the composition of the resulting product. So much uncertainty existed in his own mind that in his earlier atomic weight tables the combination of nitrous gas with oxygen was not indicated. He showed however that one volume of oxygen would combine with either one or two volumes of nitrous gas, and even then he did not include the composition of nitrous acid in his table.

It would seem therefore that this experiment did not suggest to Dalton the idea of the atomic theory, but

that he had long previously had in mind the assumption that various mathematical relationships existed between the different oxides of carbon and nitrogen. This assumption he sought to establish by reference to analyses performed both by himself and other chemists. We are thus forced to the conclusion that Dalton's theory was not founded upon experimental facts as an attempt to explain them, but rather existed in his mind as a preconceived notion, for the justification of which he turned to the results of experiment.

By this process of thought Dalton laid the foundation-stone of the mighty structure of modern chemistry. His theory has so far stood the test of time and prevailed against opposed views. Above all, it has marvellously fulfilled the duties of an hypothesis in suggesting new fields for work and new incentives to persevere in attempting to make clear the laws which govern matter.

CHAPTER VIII

THE ATOMIC THEORY (*continued*)

True to his conservative nature, Dalton failed to appreciate the advantages to be derived from a simpler system of chemical nomenclature than the one which he himself employed. Berzelius in 1815 suggested the system at present in use which to our thinking is much less cumbrous than the one involving the use of circular symbols, but to Dalton they were "horrifying" and appeared like "a chaos of atoms" calculated "to equally well perplex the adepts of science, to discourage the learner, as well as to cloud the beauty and simplicity of the Atomic Theory." It may reasonably be argued that, had Dalton been alive to-day, and been acquainted with some of the complex carbon compounds known to us, he might have modified his summary judgment on the Berzelian notation.

With the same want of consideration he dismissed Wollaston's equivalents, and we cannot offer any explanation of this lack of temperate judgment on the part of one whose thoughts were of the inductive order. It may be that his abrupt Quaker nature was too strong and self-opinionated to allow of due consideration of the claims of others.

Still more striking, however, is his refusal to appreciate the enormous significance of the law established by

Gay-Lussac. This law, still known by the name of its enunciator, states that when gases take part in a chemical change, the reacting volumes bear a simple relation to each other and to the volume of the resulting product.

This discovery affords such striking confirmation of the correctness of Dalton's views that it seems well-nigh incomprehensible that he should refuse to accept its truth. In the second part of the *New System*, he states that Gay-Lussac's "opinion is founded upon a hypothesis that all elastic fluids combine in equal measures, or in measures that bear some simple relation one to another, as 1 to 2, 1 to 3, 2 to 3, etc.; in fact, his notion of measures is analogous to mine of atoms; and if it could be proved that all elastic fluids have the same number of atoms in the same volume, or numbers that are as 1, 2, 3, etc., the two hypotheses would be the same, except that mine is universal, and his applies only to elastic fluids. Gay-Lussac could not see that a similar hypothesis had been entertained by me and abandoned as untenable."

It is clear that Dalton was aware of Gay-Lussac's discovery almost as soon as it was published, for in a letter to his brother dated 11th December 1809 he mentions that Berthollet had sent him the "Memoirs de la Société d'Arcueil" in which Gay-Lussac's communication was printed. Further his attention was drawn to the significance of the results of this work by Dr. Thomas Thomson, who says in a letter to Dalton:—

"The most important paper respecting your atomic

theory is by Gay-Lussac. It is entirely favourable to it."

Berzelius, who had readily accepted Dalton's atomic hypothesis, points out to Dalton the great support which the theory derives from Gay-Lassac's conclusions. In reply to a letter dated August 1st, 1812, Dalton writes to Berzelius as follows :[1] —

"The French doctrine of *equal measures* of gases combining, etc., is what I do not admit, understanding it in a mathematical sense. At the same time, I acknowledge there is something wonderful in the frequency of the approximation.

"The doctrine of definite proportions appears to me *mysterious* unless we adopt the atomic hypothesis. It appears like the *mystical ratios* of Kepler, which Newton so happily elucidated."

Earlier in the same letter he had expressed an opinion that the red oxide of lead was a mixture of the yellow and brown oxides combined, and to these two points (amongst others) Berzelius replies in the following words : [2]—

"Votre opinion que le minium est une combinaison de l'oxide noir avec l'oxide jaune, est peut-être fondée sur la difficulté de concevoir un demi-atome ; je crois qu'il faut laisser les expériences maturer la théorie. Si celle-ci commence à s'occuper de presser la nature dans les formes, elle cessera d'être utile et de se perfectionner. Vous avez raison en ce que la théorie des proportions multiples est une mystère sans l'hypothèse atomistique,

[1] *A New View of Dalton's Atomic Theory*, p. 159.
[2] *loc. cit.*, p. 161.

et autant que j'ai pu m'apercevoir tous les résultats gagnés jusqu'ici contribuent à justifier cette hypothèse. Je crois cependant qu'il y a des parties dans cette théorie, telle que la science vous la doit à présent, qui demandent à être un peu altérées. Cette partie p. ex. qui vous nécessite de déclarer les expériences de Gay-Lussac sur les volumes des gases qui se combinent, pour inexactes. J'aurois cru plûtot que ces expériences étoient la plus belle preuve de la probabilité de la théorie atomistique, et je vous avoue d'ailleurs que je ne croirai pas si aisés ment Gay-Lussac en défaut, surtout dans une matière où il ne s'agit que de mesurer bien ou mal."

Dalton apparently never believed in this volume law, as he could not reconcile the facts with his notion that "no two elastic fluids agree in the size of their particles." He speaks of the "French doctrine of equal measures . . . understanding it in a mathematical sense." Curiously enough, in spite of the fact that his results were less reliable than those of Gay-Lussac, Dalton was, "in a mathematical sense," nearer the truth than his rival, for recent research has shown that the molecular volumes near their critical temperatures are not equal, and Scott has established the fact that hydrogen and oxygen do not combine exactly in the ratio of two volumes to one volume.

It must not be supposed that Dalton's views met with universal acceptance. Sir Humphry Davy hesitated for long before accepting the atomic hypothesis. In 1810 he writes to Dalton and says:—"I shall be sorry if you introduce into your rising system an hypothesis which cannot last concerning the alkali metals."

A year later, Davy once more expresses dissent from Dalton's conclusions and protests "against the interpretations he (Dalton) has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views. . . . It is impossible not to admire the ingenuity and talent with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms, but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest."

Dr. T. C. Hope, between whom and Dalton a considerable amount of correspondence took place was another scientist who did not agree with all the conclusions of the atomic hypothesis, as is well seen in the following letter to Dalton.

"2nd, *January* 1811.

"DEAR SIR,—Accept of my best thanks for the copy of your second volume, which Mr. Holland conveyed to me. I should have returned my thanks sooner, but I was unwilling to acknowledge its arrival till I could say I had carefully perused the work. I have done so both with much interest and advantage. You have increased our stock of chemical knowledge by many valuable facts.

"I need not conceal from you that I am by no means a convert to your doctrine and do not approve of putting the result of speculative reasoning as experiment.

"Still, however, I admire the ingenuity of your speculations, and the happy adjustments of its subordinate parts.

"It must be gratifying to you to see your doctrines adopted by the first names in the chemical world.

"With sentiments of respect.—I am, dear sir, your very obedient servant, THOS. CHAS. HOPE."

Little by little, the philosophers adopted the views set forward by Dalton. Wollaston in his experiments on the "Superacid and Subacid Salts" states that he regards his facts as "but particular instances of the more general observations of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms. However, since those who are desirous of ascertaining the justness of this observation by experiment may be deterred by the difficulties that we meet with in attempting to determine with precision the constitution of gaseous bodies, for the explanation of which Mr. Dalton's theory was first conceived, and since some persons may imagine that the results of former experiments on such bodies do not accord sufficiently to authorise the adoption of a new hypothesis, it may be worth while to describe a few experiments, each of which may be performed with the utmost facility, and each of which affords the most direct proof of the proportional redundance or deficiency of acid in the several salts employed."

Mitscherlich states his views in the following terms:—

"This hypothesis, like every other, must undergo changes in proportion as observations are multiplied. It is possible, though highly improbable, that it may be

wholly superseded by another; yet the history of science can adduce scarcely any law, and certainly no theory, which has conducted the inquirer to so many discoveries as this hypothesis."

With the later developments of the atomic theory we have no concern in this work. Many eloquent tributes to the genius of Dalton and to the far-reaching nature of his hypothesis have been paid by scientists of many nations. One at least may be given, that of Wurtz:[1]—

"Mais l'interpretation théorique faisait encore défaut. Elle découle des travaux d'un savant anglais qui a doté la science de la conception à la fois la plus profonde et la plus féconde parmi toutes celles qui ont surgi depuis Lavoisier. Au commencement de ce siècle la chemie était professée à Manchester par un homme qui joignait à un amour ardent de la science, cette noble fierté du savant qui sait préférer l'indépendance aux honneurs, et à une vaine popularité la gloire des travaux solides. Ce professeur est Dalton; son nom est un des plus grands de la chimie."

In reviewing Dalton's scientific work, we cannot fail to be impressed with the fact than he was a thinker rather than an experimenter. His great hypothesis would seem to have been the result of physical considerations and philosophical speculations as to the nature of matter, and not of his experimental work. The researches conducted after his hypothesis became known served to confirm its accuracy. The rule of Avogadro and the work of Cannizzaro followed as necessary consequences of his theory. No radical change in our

[1] *Histoire des Doctrines Chimiques, Discours preliminaire*, p. xiv.

conceptions of the laws of chemical combination has been introduced. Dalton's experimental work was not accurate; it was greatly inferior to the marvellously detailed analysis of Berzelius. He made no discoveries which can at all compare with those of Gay-Lussac or Davy, yet in spite of all these deficiencies he has claims to the title given him by Dr. Henry—a law-giver of chemical science.

CHAPTER IX

DALTON'S LECTURES AND LATER SCIENTIFIC WORK

After the publication of his most important work on the nature of matter, Dalton continued his scientific researches on subjects of varied interest. Few things came amiss to him as affording opportunity to exert his powers of reasoning and experiment, as may be judged from a survey of the titles of various communications which he made to the Manchester Literary and Philosophical Society.

Between the years 1803 and 1820 he published many papers dealing with chemical, physical, meteorological, and physiological subjects. For example we have communications on "The Law of Expansion of Elastic Fluids, Liquids and Vapours," on "Respiration and Animal Heat," on "Fog," on "Alloys, particularly those of Copper and Zinc, and Copper and Tin."

Nor did his scientific work end with these, for in 1803 he was invited to deliver a course of lectures at the Royal Institution, and before the highest court of appeal in matters scientific he stated his views on chemistry and physics. This course of lectures began in December 1803 and was again delivered in 1809.

It appears that Dalton as a lecturer had few of those graces which serve to attract the multitude. Dr. Davy, the brother of Sir Humphry Davy says of him :—

"Mr. Dalton's aspect and manner were repulsive. There was no gracefulness belonging to him. His voice was harsh and brawling; his gait stiff and awkward; his style of writing and conversation dry and almost crabbed."

It has been suggested that *brawling* is an epithet which was quite inapplicable to a man of Dalton's retiring disposition, and that *drawling* would be a more accurate description of his voice and manner of speaking. In other ways, too, he seems to have incurred the disapproval of some members of his audience, for a writer in the *Quarterly Review* [1] complains of the awkward and unsuitable terms in which he spoke of the elements and of chemical processes in general. This writer speaks of Dalton as a lecturer in the following terms which are scarcely flattering to the philosopher:—

"His voice was harsh, indistinct, and unemphatical, and he was singularly wanting in the language and power of illustration, needful to a lecturer on these high matters of philosophy, and by which Davy and Faraday have given such lustre to their discoveries. Among other instances of his odd appropriation of epithets, we recollect that in treating of oxygen, hydrogen, nitrogen, etc., those great elements which pervade all nature, he generally spoke of them as '*these articles*', describing their qualities with far less earnestness than a London linen-draper would show in commending the very different *articles* which lie on his shelves."

In spite of these drawbacks his lectures appealed to

[1] *Quarterly Review*, Vol. XCVI.

the scientifically minded amongst his audience, and in various letters he describes his experiences. In one, written to Mr. John Rothwell in January 1804, he mentions his first meeting with Davy and the help he derived from him.

"LONDON, *January* 10th, 1804.

"I was introduced to Mr. Davy, who has rooms adjoining mine (in the Royal Institution); he is a very agreeable and intelligent young man, and we have interesting conversations in an evening; the principal failing in his character as a philosopher is that he does not smoke. Mr. Davy advised me to labour my first lecture; he told me the people here would be inclined to form their opinion from it; accordingly, I resolved to *write* my first lecture wholly; to *do* nothing but to tell them what I would do, and enlarge on the importance and utility of science. I studied and wrote for near two days, then calculated to a minute how long it would take me reading, endeavouring to make my discourse about fifty minutes. The evening before the lecture, Davy and I went into the theatre; he made me read the whole of it, and he went into the furthest corner; then he read it and I was the audience; we criticized upon each other's method. Next day I read it to an audience of about 150 or 200 people which was more than were expected. They gave a very general plaudit at the conclusion, and several came up to compliment me upon the excellence of the introductory. Since that I have scarcely written anything: all has been experiment and verbal explanation. In general

my experiments have uniformly succeeded, and I have never once faltered in the elucidation of them. In fact, I can now enter the lecture room with as little emotion nearly as I can smoke a pipe with you on Sunday or Wednesday evening."

Notwithstanding his evident pride in the success of the experiments he carried out before his distinguished audience, he seems to have been greater as a philosopher and law-giver than as a practical demonstrator of natural phenomena, for Dr. Henry says of him "he never possessed the art of devising or executing impressive illustrative experiments; and in my remembrance, as often failed as succeeded in performing those elementary experiments which he did attempt."

A more complete account of his impressions of lecturing is to be found in a letter to his brother written on 1st February 1804, after his return to Manchester. This letter is as follows:—

"DEAR BROTHER,—I have the satisfaction to inform thee that I returned safe from my London journey last seventh day, having been absent six weeks. It has on many accounts been an interesting *vacation* to me, though a very laborious one. I went in a great measure unprepared, not knowing the nature and manner of the lectures in the Institution, nor the apparatus. My first was on Thursday, December 22, which was introductory, being entirely written, giving an account of what was intended to be done, and natural philosophy in general. All lectures were to be one hour each, or as near as might be. The number attending were

from one to three hundred of both sexes, usually more than half men. I was agreeably disappointed to find so *learned* and *attentive* an audience, though many of them of rank. It required great labour on my part to get acquainted with the apparatus, and to draw up the order of the experiments, and repeat them in the intervals between the lectures, though I had one pretty expert to assist me. We had the good fortune, however, never to fail in any experiment, though I was once so ill-prepared as to beg the indulgence of the audience, as to part of the lecture, which they most handsomely and immediately granted me by a general plaudit. The scientific part of the audience was wonderfully taken with some of my original notions relative to heat, the gases, etc., some of which had not before been published. Had my hearers been generally of the description I had apprehended, the most interesting lectures I had to give, would have been the least relished,—but as it happened, the expectation formed had drawn several gentlemen of first-rate talents together; and my eighteenth, on heat and the laws of expansion, etc., was received with the greatest applause, with very few experiments. The one that followed, was on *mixed elastic fluids*, in which I had an opportunity of developing my ideas, that have already been published on the subject, more fully. The doctrine has, as I apprehended it would, excited the attention of philosophers throughout Europe. Two journals in the German language came into the Royal Institution, whilst I was there, from Saxony, both of which were about half filled with translations of the papers

I have written on the subject, and comments upon them.

"Dr. Ainslie was occasionally one of my audience, and his sons constantly; he came up at the concluding lecture, expressed his high satisfaction, and he believed it was the same sentiment with all or most of the audience. . . . I was at the Royal Society one evening, and at Sir Joseph Banks's another. This gentleman I had not, however, the pleasure of seeing, he being indisposed all the time I was in London.

"I saw my successor, William Allen, fairly launched; he gave his first lecture on Tuesday preceding my conclusion. I was an *auditor* in this case, the first time, and had an opportunity of surveying the audience. Amongst others of distinction the Bishop of Durham was present. . . . In lecturing on optics I got six ribands, blue, pink, lilac, and red, green, and brown, which matched very well, and told the audience so. I do not know whether they generally believed me to be serious, but one gentleman came up immediately after and told me he perfectly agreed with me; he had not remarked the difference by candle-light."

This course of lectures was repeated at the Royal Institution in 1809-1810, and some of Dalton's lecture notes have been preserved and published.[1] The interest attaching to them is sufficient to justify certain quotations being made from them, and they will further serve to illustrate the methods he adopted in lecturing.

In the fifteenth lecture, of January 23rd, 1810,

[1] *A New View of the Origin of Dalton's Atomic Theory*, p. 99, *et seq.*

Dalton discusses the subject of heat. His lecture notes have been preserved and are here quoted in full as being of considerable interest.

"*Lecture* 15.—On Heat.

"*23rd January* 1810.—The very general importance of facts and experiments relating to heat will be admitted by all who are acquainted with the mechanical arts and with physical science. Facts and experiments, however, relating to any subject, are never duly appreciated till, in the hand of some skilful observer, they are made the foundation of a theory by which we are able to predict the results and foresee the consequences of certain other operations which were never before undertaken. Thus a plodding experimentalist of the present time, in pursuit of the law of gravitation, might have been digging half way to the centre of the earth in order to find the variation of gravity there, were it not that the sublime speculations of Newton have already anticipated the result, and spared him the undertaking of a fruitless and endless labour.

"In reference to the subject before us, it has been found that if a quantity of any elastic fluid be compressed by *mechanical* force, its temperature is raised, or it parts with a quantity of its heat—and it recovers the same quantity of heat again upon being liberated, and the same volume as it previously possessed. This is a particular fact. Again, it has been found that a piece of iron may be hammered till it is red-hot; that it is condensed in volume by the operation; and that it does not, like the air, recover its former heat and former

volume of its own accord, but requires to be heated red-hot again and cooled slowly before it resumes its original temperament and volume. This is another fact. But of how much more importance would it be to ascertain from these and such like facts that it is an universal law in nature that whenever a body is *compressed*, whether by *mechanical* or *chemical* agency, it loses a portion of its heat; and whenever a body is *dilated* it *gains* a portion of heat from other bodies?

"Now, whether this is in reality a law of nature is not yet, perhaps, clearly ascertained; but this is certain that a person apprehending such a law is more likely to have a proper bent given to his investigations than one who makes a number of experiments without any fixed object in view.

"I have made these observations to show that, however guarded we should be, not to let a theory or hypothesis, contradicted by experiment, mislead us; yet it is highly expedient to form some previous notion of the objects we are about, in order to direct us into some train of enquiry.

"The doctrine of heat is justly considered as constituting a fundamental part of chemical science. It constitutes the basis of mechanical power in that most useful instrument, the steam engine. The cause of animal heat forms one of the most important enquiries in physiology. Even in the mechanical arts the knowledge of heat is necessary. The clock and watchmakers are not competent to their arts if they do not understand the laws of expansion by heat. We have known instances of very large castings of iron having been pulled to pieces

by their own reaction, before ever they were cooled, by the careless or injudicious manner of cooling them. And everyone knows the nice attention to temperature that is requisite in manufacturing and working with glass.

"It would be endless to enumerate the arts, sciences, and manufactures in which a knowledge of the nature and laws of heat is advantageous and even expedient. If the economy of fuel was the only object in pursuit in these speculations it would be well deserving our attention.

The *first* question that naturally arises for our discussion is—

"*What is Heat?*

"Were we to proceed as the experimental philosopher must do—that is, in the *analytical* mode of investigation, this question would most properly be the *last*. But as we profess to teach or instruct from the knowledge we have previously acquired, or in the *synthetical* way, this question may properly be considered at the outset.

"1. Theory.—Heat a distinct and peculiar elastic fluid *sui generis*; its leading features are to be *repulsive* of its own particles; and *attractive* of those of other matter . . . quantity incapable of change.

"2. Theory.—Heat is a quality of bodies—consists in some kind of vibration of the particles—is, like mechanical forces, communicated from one body to another; is incapable of change in quantity, like the force of elastic bodies.

"3. Theory.—Heat is a quality of bodies—consists in some kind of vibration of the particles—and is capable of being *generated* or *destroyed*, consequently

is capable of change in quantity in different bodies without reciprocal communication.

"Animadversions on these theories :—

"Refer to diagram.

"Refer to a candle burning.

"Refer to ordinary combustion.

"Refer to the heat of the human body.

"Refer to freezing water.

"This leads to *capacity* for heat: refer to two equal electric jars, one thick, the other thin;

"And to capacity as applied to the human intellect—thick skull and thin.

"*Quantity* of heat and *intensity* of heat considered: refer to water and mercury. See diagram of capacity.

"Whatever theory of heat we adopt we must allow of *quantity* and *intensity* of heat.

"Measure of *intensity* or temperature by the thermometer :—

"Investigations of De Luc, Crawford, etc., concerning the thermometer :—

"De Luc finds mean 110° } instead of 122°.
Crawford ,, ,, 121° }

"Principle wrong that equal weights (or equal bulks) of water mixed at any different temperature should give the mean. Reason assigned. Error, 8° above the mean.

"These considerations induced me to try some other method :—

"1. Discovery that water and mercury expand by the same law—namely, the expansion as the square

of the temperature from the point of greatest density or congelation. See diagrams.

"2. That the force of steam is in geometrical progression to equal increases of temperature or *intensity* of heat. Instances in steam of water and ether.

"3. That the expansion of air is in geometrical progression to equal increments of temperature.

"Animadversions on former experiments on the expansion of gases, General Roi, Morveau, etc. See diagram.

"4. Refrigeration of bodies in geometrical progression to equal increments of temperature.

"This law suspected by Newton, but found not to answer by the common thermometer.

"Experiment.—If a thermometer be heated to 400° above the temperature of the air and suffered to cool, it will be found:—

"400°
200° in 2 minutes of time.
100 ,, 2 ,, more.
50 ,, 2 ,, ,,
25 ,, 2 ,, ,,
$12\frac{1}{2}$,, 2 ,, ,, etc., etc.

"Here the quantity of heat thrown off in any given time is as the *intensity* or excess of temperature, as it ought to be. Hence the thermometer so graduated must be concluded to be an accurate measure of *intensity* of heat, or, as it is more frequently called, *temperature*.

"The agreement of these four important phenomena in establishing a great fundamental law of heat is such as must carry conviction to everyone who will take the

trouble duly to consider them—the only doubt that can exist is as to the accuracy of the *facts*. This at present may be said to rest entirely on my authority, though it need not in fact do so. I might appeal to Bétancourt in regard to steam, to General Roi in regard to expansion of air, to Blagden in regard to expansion of water. The experiments on the refrigeration of bodies are more peculiarly mine. They are, however the most easily made, etc.

"It is now one and a half year since these results were before the public. No animadversions on them in this country, notwithstanding their evident importance. Perhaps the repetition of the experiments, and the confirmation of them, may be soon expected from France, etc. If this should be the case we may well exclaim—Where are the *descendants* of *Newton*, of *Bacon*, of *Hooke*, and of *Boyle*, that the merits or demerits of the productions of *Englishmen* cannot be ascertained in their own country? I trust and hope, for the honour of my country, that this *inattention* to new and important views on a subject of so much consequence as that of heat, is somehow or other *accidental* or *apparent only*, and is not to be ascribed to the want of ardour in the pursuit of science, or to the incompetency of those whom the public look up to as judges and authorities."

These words, written almost a century ago, have to-day perhaps a deeper significance than when first penned, and within the last few years there has not been wanting adequate expression of the sense of failure to grasp the importance of new scientific discoveries. The trust and hope which Dalton possessed have not

been altogether brought to fulfilment. It remains yet to be seen whether Englishmen will take away from themselves the reproach of "inattention" to matters which must affect their national prosperity and well-being.

In the sixteenth lecture he continued to discuss the subject of *Heat* and drew comparisons between heat, electricity and light.

The seventeenth lecture dealt with the chemical elements, and the notes for this have already been given in full (p. 114). The next three lectures are concerned with the same subject, and deal with the elements and their combinations. In Lecture XVIII., he writes of the elements in the following terms, showing that his conception of matter is quite different from that of those philosophers who maintained that "however unlike" it was "always the same thing." He believed that this latter was not Newton's view and says:—

"I should apprehend there are a considerable number of what may be properly called *elementary* principles, which never can be metamorphosed, one into another, by any power we can control. We ought, however, to avail ourselves of every means to reduce the number of bodies or principles of this appearance as much as possible; and after all we may not know what elements are absolutely indecomposable, and what are refractory, because we do not apply the proper means for their reduction."

The notes for the remaining lectures contain accounts of various compounds examined with a view to determining the proportions in which the elements combine,

always with the object of confirming his theory, of establishing "that beautiful and simple theory of chemical synthesis and analysis" which he adopted "from a conviction of its application to the general phenomena of chemistry."

Dalton's views on the nature of gases did not pass unchallenged. Dr. T. C. Hope engaged in a somewhat lengthy correspondence with him on the subject, offering objections to which Dalton replied in his *New System*. In other subjects he seems to have taken an equally great interest, as for example the phenomena of respiration, animal heat, and other matters having a physiological trend.

A few of his more purely chemical papers may be briefly noticed, as they will be of service in illustrating his powers as an experimental chemist. Sometimes he is singularly careless in his work, as in the case of the researches on the absorbent power of charcoal. He makes the curious mistake of heating the charcoal to redness in order to expel air and moisture and then pulverises it, allowing it at the same time to remain in contact with the atmosphere. After this process he found that the charcoal was incapable of absorbing any but the smallest traces of carbon dioxide. On the other hand he sometimes succeeded in obtaining results which were comparatively accurate. This is seen in his work on sulphuric ether which he embodied in a paper read before the Manchester Literary and Philosophical Society on 16th April 1819.[1] In this communication he describes the preparation of the sulphuric ether and its

[1] Mem., Lit. and Phil. Soc., Manchester, Vol. III. p 446.

separation from alcohol. He found its specific gravity to be 0·720 and its boiling point 95° to 96° F. Further he made an analysis by exploding ether vapour with oxygen in a Volta's eudiometer and found that ten volumes of ether vapour required sixty volumes of oxygen for combustion, the volume of the resulting carbon dioxide being forty volumes. That these results are accurate is at once obvious from an inspection of the equation

$$C_4H_{10}O + 6O_2 = 4CO_2 + 5H_2O.$$

Dalton still persisted in recording meteorological observations such as the dew point, temperature, and barometric pressure, and during his holidays in Lakeland he always managed to find material for thought and study. During a period of over twenty years he recorded each year the dew-point at three different stations, viz., in the valley at the foot of Helvellyn, at Brownrigg Well and at the summit of Helvellyn. From the statistics thus obtained he concluded that "the quantity and density of vapour is constantly (or with very rare exceptions) less the higher we ascend."

Gas from the floating island in Derwentwater was collected and subsequently analysed by Dalton who found that it was "half carburetted hydrogen and half azote, with a slight portion of carbonic acid gas."

He constantly employed the barometer for the purpose of ascertaining altitude, and the results obtained were generally in fair agreement with those of the ordnance survey, though as a rule slightly higher than the true values.

His later scientific papers were not of profound interest, and of small value compared with that of his work on the atomic hypothesis. In 1840 he communicated to the Royal Society an essay on the Phosphates and Arseniates, which the Council declined to publish because of its obscurity. Dalton was highly indignant and published this in separate form, "with the indignant comment, 'Cavendish, Davy, Wollaston, and Gilbert are no more.' He concludes, 'I intend to print my essays in future, to be appended to my other publications. Some of them are materially affecting the atomic system.'"[1]

The essays, four in number, to which he alludes, were published later. The titles were "On Microcosmic Salt," "On the mixture of Sulphate of Magnesia and the Biphosphate of Soda," "On the quantity of Acids, Bases and Water in the different Varieties of Salts, with a new method of measuring the water of crystallisation, as well as the acids and bases," and "On a new and easy Method of Analysing Sugar."

In the third paper he describes what he considers to be the "greatest discovery that I know of next to the atomic theory." This discovery was connected with the behaviour of anhydrous salts in solution. He showed that certain dehydrated salts cause no increase of volume when dissolved in water, and that hydrated salts on solution caused an increase in volume equal in quantity to the volume of the water of crystallisation. The universal truth of this phenomenon has been disproved and Dalton's view, that "only the water adds

[1] Henry's *Life of Dalton*, p. 193.

to the *bulk*, and the solid matter adds to the *weight*," has been shown to be erroneous.

His scientific labours showed a distinct deterioration in quality during the later years of his life, and although he attacked many problems and showed much ingenuity in dealing with their difficulties, it cannot be said that the work of his maturer years was of the same excellence as that by which he is best known.

CHAPTER X

DALTON IN LATER LIFE

Dalton was in no sense a man of the world. He did not appear to the best advantage in society. His manners were in no way polished, and he lacked the qualities which are essential to social success. He was certainly no respecter of persons, a fact amply proved by many stories told of his brusque manner.

On one occasion a distinguished Frenchman visited Manchester with the sole object of seeing Dalton. Greatly to his surprise, he found the philosopher engaged in giving an arithmetic lesson to a boy. The visitor asked if it was M. Dalton that he had the honour of addressing. "Yes," replied Dalton, "will you sit down till I put this lad right about his arithmetic."

Another characteristic story is told concerning his presentation at the court of William the Fourth. Dalton as a Quaker could not appear in court dress wearing a sword, but the difficulty was obviated by his wearing the scarlet robe of a Doctor of Laws of Oxford. The king observed the strange dress and asked "Who is that?" An official of the court informed His Majesty that it was Dr. Dalton, the great Manchester philosopher. The king at once addressed him and said, "Well, Dr. Dalton, how are you getting on in Manchester—

all quiet, I suppose?" "Well, I don't know," replied Dalton, "just middlin', I think."

But though he did not shine as a social light, he was far from awkward or shy amongst his intimate acquaintances. Though inclined to be silent in the company of others, even in that of his friends, he sometimes allowed himself to unbend so far as to make a joke or to laugh at some sally. He took great pleasure in the visits which fellow scientists paid him, and Miss Johns records the names of several distinguished visitors who came to see him. In her diary, to which source we are chiefly indebted for the facts concerning Dalton's social life she says, speaking of these visitors:—

"Amongst them, I particularly remember M. Biot, as the most interesting person possible. . . . A Bavarian who died in Scotland, poor man! A Pole from Wilna, who mentioned the distress that the march of Napoleon's army had occasioned, by causing bread to rise to 2s. 6d. per pound at Wilna. Perhaps I remember most vividly the visit of Dr. Forchammer, a Dane, a most sensible and agreeable person, whom my father thought the most accomplished person he had ever met with, and whom the doctor described as profoundly versed in chemistry, and other branches of natural philosophy. . . . The doctor, after his illness particularly, took great pleasure in recalling the remembrance of the distinguished persons, both at home and abroad, whom we had had the privilege of knowing and conversing with."

After his lectures at the Royal Institution, Dalton travelled rather more than had been his custom previously, and apparently in the course of his travels

he lost some of that shy awkwardness which was characteristic of his earlier days. This change in habits and manners is no doubt partly to be attributed to his having met with men more at ease in the social world, men whom Dalton could admire as philosophers and who yet possessed the saving grace of *savoir faire*. Such men were Davy, Wollaston, and Adam Sedgwick whose names are household words, and who were even in Dalton's time famous as thinkers.

In 1807 he delivered in Edinburgh a course of lectures similar in scope and character to those already given at the Royal Institution. He prefaced this course with the following introduction:—

"It may appear somewhat like presumption in a stranger to intrude himself upon your notice in the character I am now assuming, in a city like this, so deservedly famous for its seminaries of physical science. My apology, however, shall be brief. The field of science is large; it is, therefore, impossible for any individual to cultivate the whole. My attention has been directed for several years past, with considerable assiduity, to the subjects of *heat*, of *elastic fluids*, of the *primary elements* of bodies and the *mode of their combinations*. In the prosecution of these studies several new and important facts and observations have occurred. I have been enabled to reduce a number of apparently anomalous facts to general laws, and to exhibit a new view of the first principles or elements of bodies and their combinations, which if established, as I doubt not it will in time, will produce the most important changes in the system of chemistry, and reduce the whole to a science

of great simplicity, and intelligible to the meanest understanding. My object in the proposed short course of lectures is to exhibit to you the grounds and reasoning on which I entertain those ideas. I choose Edinburgh and Glasgow in preference to any other cities in Britain because I apprehend the doctrines inculcated would in those cities meet with the most rigid scrutiny, which is what I desire. This is my only apology."

Although the main object of his visit to Edinburgh was for the purpose of lecturing on the subject he had made his own, this did not prevent him from making the acquaintance of prominent members of the university and city, or from sight-seeing, as will be seen in the following letter he wrote from that place to his old friend the Rev. W. Johns :—

"EDINBURGH, *April* 9th, 1807.

"RESPECTED FRIEND,—As the time I proposed to be absent is nearly expired, and as my views have recently been somewhat extended, I think it expedient to write you for the information of enquirers. Soon after my arrival here I announced my intention by advertisement of handbills. I obtained introduction to most of the professional gentlemen in connection with the college, and to others not in that connection, by all of whom I have been treated with the utmost civility and attention. A class of eighty appeared for me in a few days. My five lectures occupied me nearly two weeks; they were finished last Thursday, and I was preparing to leave the place and return by Glasgow to

spend a week. But several of the gentlemen who had attended the course represented to me that many had been disappointed in not having been informed in time of my intention to deliver a course, and that a number of those who had attended the first course would be disposed to attend a second. I have been induced to advertise for a second, which, if it succeeds, will commence on Wednesday, the 22nd, and be continued daily. This will detain me a week yet; I then set off for Glasgow, where I may be detained a week or more, so that I see no probability of reaching Manchester before the beginning of May, to which I look forward with some anxiety. Hitherto I have been most highly gratified with my journey; it is worth coming a hundred miles to see Edinburgh. It is the most romantic place and situation I ever saw; the houses touch the clouds. At this moment I am as high above the ground as the cross on St. James's spire; yet there is a family or two above me. In this place they do not build houses side by side as with you; they build them one upon another—nay, they do what is more wonderful still: they build one street upon another, so that we may, in many places, see a street with the people in it directly under one's feet, at the same time that one's own street seems perfectly level and to coincide with the surface of the earth. My own lodgings are up four flights of stairs from the front street, and five from the back. I have just one hundred steps to descend before I reach the real earth.

"I have a most extensive view of the sea. At this moment I see two ships, and mountains across the Firth

of Forth, at a distance of thirty miles. To look down from my windows into the street at first made me shudder, but I am now got so familiar with the view that I can throw up the window and rest on the wall, taking care to keep one foot as far back in the room as I can to guard the centre of gravity. The walks about Edinburgh are most delightfully romantic. The weather is cold; ice every morning, and we had a thick snow a few days ago. Upon walking up on to an eminence I observed all the distant hills white, the nearer one speckled. I saw five or six vessels just touching the horizon; they seemed to be about ten or twelve miles off, and their white sails looked like specks of snow on the sea. I saw a dozen or two at anchor in the river, and a most charming view of the Fifeshire hills on the other side of the Firth. Adieu. My best regards to you all. JOHN DALTON."

Thus we see that Dalton, like many others, yielded to the charms of the wonderful city, and found a beauty in its terraced streets of high houses, in the distant views of sea and mountains; like others he experienced its climatic terrors. He certainly never failed to appreciate the beauties of nature, and in his letters we constantly find descriptions of scenery that appealed to his æsthetic sense. In his travels, whether to some hitherto unvisited town or city, or to his native hills and dales, he generally found something worth recording. His writing, too, was not always "dry and almost crabbed" as Dr. Davy described it, for he occasionally lapses into the descriptive style as in the following passage where

he is giving his impressions of a scene near Ambleside:—"The atmosphere was as clear as possible; Jupiter and the fixed stars shone with uncommon splendour, and suggested an unusual proximity. The moon, risen, but not above the mountains cast a glimmering light upon the rocky hills just opposite, and produced a fine effect. These circumstances, together with the awful silence around, would have persuaded us we had been transferred to some other planet."

He even so far left the high walks of philosophy as to write verse, though his most ardent admirers can scarcely dignify him with the name of poet if, as Dr. Henry states, the "Stanzas addressed to an Eolian Lyre" is the most favourable specimen of his efforts in this direction.

Moreover he was susceptible to the influence of music and went so far as to assist another Friend in drawing up a petition to the yearly meeting, "soliciting permission to use music under certain limitations."

Throughout his life he remained the simple, unaffected north-countryman and Quaker, wanting in academic culture, and, as far as is known, almost entirely unacquainted with *belles lettres* and historical subjects. His manners lacked polish and those little touches of refinement which enabled men like Davy, Wollaston, and others of his contemporaries to rise to positions of eminence.

With reference to his want of general reading a curious story is told of him which shows how so clear-sighted a man, as he proved himself to be, may yet stumble against obstacles of his own raising. Not only

did he never make wide use of books, but he actually discouraged in others the use of them. He strenuously opposed the formation of a good scientific library in connection with the Manchester Literary and Philosophical Society, saying in support of his views, "I could carry all the books I have ever read on my back." Though probably not literally accurate, this statement suffices to show how in one respect he failed to make the most of his undeniably great abilities; and yet at the same time this lack of knowledge of the opinions and thoughts of others had probably much to do with the striking originality and boldness of his ideas. His views on any given subject were seldom conventional; in thinking he was not hampered by any school or system of philosophy, but always sought to explain phenomena by the light of his own wisdom, and was at the same time inclined to be intolerant of the views of other workers.

In speech he was sometimes more candid than good manners allow. After listening to a long paper on some subject not concerned with his own studies he made the remark, "Well, gentlemen, I daresay this paper is very interesting to those who take an interest in it." Such words from the President of a learned society seem hardly fitting and yet they are in perfect keeping with the abrupt manners of a man with no pretence to good breeding, and of one who prided himself on always giving direct utterance to his thoughts.

Dalton objected to catechism on scientific matters, and on one occasion replied rather irritably to a seeker after knowledge, "I have written a book on that sub-

ject, and if thou wishest to inform thyself about the matter, thou canst buy my book for 3s. 6d."

Though abrupt in speech and apparently discourteous in manner he was not unkindly, and towards women, according to Miss Johns, he showed "those little delicate attentions which are particularly acceptable to them." She goes on to say :—

"As one instance of this kind, I may mention that my sister and myself never left home, be the hour ever so untimely, that he did not see us into the coach and safe off. But not only to us, to the servants also did he pay the same kind attentions on their annual visits to see their friends. These servants had lived for many years in the family, of which they were considered and treated as a part. His conduct to them was uniformly kind, considerate and liberal. When we left Manchester, before taking leave, he called these two women, and, highly praising their fidelity and constancy in remaining for so many years in the same place, he gave to the elder one two, and to the younger one, one sovereign."

Dr. Henry quotes an instance of his generosity related to him by a lady who knew Dalton intimately.

"A fire occurred in the works during my brother's minority. A few days afterwards, Mr. Dalton offered to my mother all the funds he had saved, if any money was wanted. It was not required; but we thought it an act of very considerate kindness and friendship."

The contributions which he made to the funds of the Society of Friends were made after due deliberation.

If the cause seemed worthy, Dalton gave generously, according to his means. He was always loyal to his friends, and when occasion arose treated them with the utmost generosity. During his later years, when in comparatively easy circumstances, he made an annual allowance to two distant relatives, and subsequently provided for them in his will.

In matters where money was concerned he was most precise, in fact in one instance this scrupulous care of trifles becomes laughable. Lord Playfair tells the anecdote in the following words :—

"At one of the meetings of the Literary and Philosophical Society of Manchester at the end of 1843, or early in 1844, I gave Dalton a small pamphlet, which was a reprint of two lectures which I had given to the Royal Agricultural Society. A few days after, Dalton sent for Peter Clare, to ask him if he knew what I had given him, as he had mislaid it. Peter Clare assured him he had received nothing from me, so the housekeeper was sent for, and she recollected that he had brought home a book with my name on it, but she was unable to say what it was. Accordingly Peter Clare was sent to my laboratory, as an ambassador, to ascertain what the book was, *and what was its value.*

"As an author, I found some difficulty in estimating the value of the performance; but was soon relieved by being told, that it was not the scientific but the *money* value of the present, which Dalton desired to know. I stated that the price, if sold, might have been sixpence or a shilling. In about an hour Peter Clare returned

with a message from Dalton, that he had intended to have presented me with all his works, but as their cost was above thirty shillings he could not think of giving them for a one shilling present. He made, however, the entertaining proposal, that I should gather together all 'I had ever written, send them to him with a priced list, and then he would make up his mind as to whether he could present me with his works.' You may suppose, that I was too much amused with the proposal to hesitate, so I gathered together what I could, including my translation of Liebig's *Agricultural Chemistry*. After this had been done, I still found that the declared value of my present was seventeen shillings less than that offered by Dalton, but nevertheless I boldly sent it. Next day came Peter Clare with all Dalton's Works, every volume having on its blank page in the author's autograph,

"John Dalton, D.C.L., F.R.S.,
to
Dr. Lyon Playfair
Jany. 1844.

"But I was immediately requested forthwith to hand over the 17s., which I immediately did, delighted to have the works from the great author, and prizing them the more from the droll characteristic way in which they came into my possession."

Sir Henry Roscoe in his life of Dalton says, "Amongst the Dalton papers, I found a memorandum about this 'transaction' in Peter Clare's handwriting. On one side is written—'John Dalton, D.C.L., F.R.S.,

etc. etc., to Dr. Lyon Playfair, Jan. 30 1844.' And on the other side :—

'Meteorology	. . . .	£0 6 0
Chemistry, first part	. . .	0 7 0
Do., second part	. .	0 10 6
Do., Vol II. Part I.	. .	0 12 0
		£1 15 6'"[1]

In justice to the philosopher, however, it is only fair to say that this incident took place a few months before his death, when his mental powers were failing. In his days of greater mental activity, he was as Dr. Henry says "always liberal, though frugal."

The chief accounts we possess of his tastes and habits we gather from Miss Johns' diary and Dalton's own letters and brief notes. The two following letters to Mr. Johns are sufficiently characteristic to be of interest. The first one, written from London in 1809 when he was delivering his second course of lectures at the Royal Institution reads as follows :—

"On Tuesday I spent greater part of the day (morning, they call it here) with Mr. Davy in the laboratory of the Royal Institution. Sir J. Sebright, M.P., who is becoming a student of chemistry, was present. On Wednesday I attended Mr. Bond's lecture on astronomy, and prepared for mine the next day. On Thursday, at two, I gave my first lecture. Mr. Pearson, a former acquaintance, went home with me after the lecture, and we had a long discussion on mechanics. Mr. Davy

[1] *John Dalton and the Rise of Modern Chemistry*, p. 192.

had invited me to dine with the club of the Royal Society at the Crown and Anchor at five o'clock; but I was detained till nearly six. I got there, and called Davy out. All was over; the cheese was come out. I went, therefore, to the nearest eating-house I could find to get a dinner. I went in and asked for a beef-steak. 'No.' What can I have? 'Boiled beef.' Bring some immediately. There was nothing eatable visible in the room, but in three minutes I had placed before me a large pewter plate covered completely with a slice of excellent boiled beef, swimming in gravy, two or three potatoes, bread, mustard, and a pint of porter. Never got a better dinner. It cost me 11½d. I should have paid 7s. at the Crown and Anchor. I then went to the Royal Society, and heard a summary of Davy's paper on Chemistry, and one of Home's on the poison of the rattlesnake; Sir J. Banks in the chair. Davy is coming very fast into my views on chemical subjects. On Friday I was preparing for my second lecture. I received a visit from Dr. Roget. On the evening I was attacked with sore throat. I sweated it well in the night with clothing, but it was bad on Saturday, and I was obliged to beg a little indulgence of my auditors on the score of exertion. However, I got through better than I expected. I kept in on Sunday and Monday and got pretty well recruited. On Tuesday I had my third lecture, after which I went to dine at a tavern to meet the Chemical Club. There were five of us, two of whom were Wollaston and Davy, secretaries of the Royal Society. We had much discussion on chemicals. Wollaston is one of the

cleverest men I have yet seen here. To-day, that is Thursday (for I have had this letter two or three days in hand), I had my fourth lecture. I find several ingenious and inquisitive people of the audience. I held a long conversation to-day with a lady on the subject of rain-gauges. Several have been wonderfully struck with Mr. Ewart's doctrine of mechanical force. I believe it will soon become a prevalent doctrine. I should tell Mrs J. something of the fashions here, but it is so much out of my province, that I feel rather awkward. I see the *belles* of New Bond Street every day, but I am more taken up with their faces than their dresses. I think blue and red are the favourite colours. Some of the ladies seem to have their dresses as tight round them as a drum, others throw them round like a blanket. I do not know how it happens, but I fancy pretty women look well either way.

"I am very regular with my breakfast, but other meals are so uncertain that I never know when or what. Hitherto I have dined at from two to seven o'clock; as for tea, I generally have a cup between nine and ten, and, of course, no supper. I am not very fond of this way of proceeding. They say things naturally find their level, but I do not think it is the case in London. I sent for a basin of soup the other day before I went to lecture, thinking I should have a good threepenny-worth, but I found they charged me one shilling and ninepence for a pint, which was not better than our Mary's broth. Of course, I could not digest much more of the soup."

In the next letter he describes how he was nearly

poisoned by lead in the porter which he drank. In all probability the lead was derived from the pipes through which the porter was drawn. He further gives an account of celebrities whom he met.

"LONDON, *January* 29, 1810.

"You may perhaps have heard from Dr. Henry that I have been nearly as ill as formerly, that I have been nearly poisoned since I came here. I had been about three weeks when I discovered it was the porter here which produced the effects. I have not had a drop since, and have never had any more of the symptoms.

"I have had a pretty arduous work, as you may imagine, having had three lectures to prepare each week, to attend two others, and to visit and to receive visits occasionally besides. I find myself just now in the focus of the great and learned of the metropolis. On Saturday evening I had a discussion with Dr. Wollaston, and a party at Mr. Lowry's. On Sunday evening, last night, I was introduced to Sir Joseph Banks, at his house, by Sir John Sebright. Sir Joseph said, 'O Mr. Dalton, I know him very well; glad to see you; hope you are well,' etc. There were forty or more of the leading scientific characters present, many of whom were my previous acquaintances, such as Sir Charles Blagden, Drs. Wollaston, Marcet, Berger, and Roget; Messrs. Cavendish, Davy, Tennant, Lawson, etc. We had conversation for about an hour or more in Sir Joseph's library, when the company dispersed. To judge from the number of carriages at the door it might be a court evee.

"I paid a visit, in company with Dr. Lowry, to Dr. Rees the other day; we spent an hour in conversation in the doctor's library. The doctor seems a worthy philosopher of the old school; his evening lucubrations are duly scented with genuine Virginia."

After this attack of lead-poisoning, Dalton was always suspicious of water from unknown sources, and frequently made tests to convince himself of the absence of lead. During one illness he feared a recurrence of this poisoning and was given a remedy by the doctor whom he consulted. He quickly recovered and his recovery, according to the doctor, was to be ascribed to the efficacy of the medicine prescribed. "I do not well see how that can be," replied the chemist, "for I kept the powder until I could have an opportunity of analysing it."

His habits in later life were as orderly and methodical as in his younger days. Mr. Giles, a friend and former pupil, thus describes his daily life:—

"In his habits and manner of life, Dr. Dalton was extremely simple, regular, methodical, orderly and temperate. He rose regularly at seven o'clock, and a few minutes after eight the writer has often seen him going across from his lodgings in George Street, or his house in Faulkner Street, which were close by (like Diogenes with lighted lantern in hand) to the rooms of the Literary and Philosophical Society. After having lighted his own fire, and dusted down the desk for his pupils, he went back to his breakfast, and was ready for his pupils at nine o'clock. From nine till twelve, or nearly one o'clock, he devoted to teaching, or in pursu-

ing his own investigations; and frequently after giving his pupils a mathematical or chemical problem to solve, he would go on with his own pursuits until the pupils required explanation, or had finished the problem they were working out. Between twelve and one o'clock he almost daily visited the Portico news-room and library, in order to watch the progress of political events, or to read any notices on scientific subjects.

"At one o'clock he went to his dinner; and after a temperate meal, at which he seldom drank anything but water, he returned to his laboratory at two o'clock, and pursued his private teaching or his own experiments until five o'clock, and generally visited the Portico, a second time in the day, before or after an early tea. At six o'clock he returned to his laboratory, until about half-past eight or nine o'clock, unless he was spending the evening with a friend; and then after a light supper, consisting generally, the last few years of his life, of sago, or oatmeal porridge, and after smoking a pipe, retired to rest at ten or latest eleven o'clock."

The same writer records the account of an interview which Dalton had with Dr. Chalmers. Dalton "expressed himself to be much gratified to find that Dr. Chalmers' views were enlarged and scientific, and not those which many divines held, viz., that the matter of which the world is made, as well as the form it now assumes, was created only 4000 years before the Christian era."

His religious life was simple. He attended regularly the meetings of the Society of Friends, but as to his views of doctrine nothing is known. Giles states that

he was rarely seen engaged in devotional exercise either private or social, but it appears that he was a firm believer in the revelation of truth found in the Bible.

In money matters he was precise but not mean. Living as he did in Manchester, where at that time as now, many of the inhabitants were engaged in the pursuit of making haste to be rich, he lived quietly and was content with the most modest remuneration. He made chemical analyses for exceedingly moderate fees. A few shillings was the average charge, and no one ever paid him more than a sovereign for this class of work.

He further increased his income by giving private lessons in chemistry and mathematics. Here, too, his fees were low, as he charged 2s. 6d. per hour, but if two pupils were taught together they each paid only 1s. 6d.

His stock of worldly goods was substantially increased at the death of his brother Jonathan in 1834, and by the bestowal of a pension of £150 per annum in 1833, an amount subsequently increased to £300.

CHAPTER XI

DALTON IN LATER LIFE (*continued*)

In the year 1810 Sir Humphry Davy requested Dalton to offer himself as a candidate for election to the Royal Society. This he declined to do, but the reason for his action has not transpired. It has been suggested that the fees payable on admission were too heavy for Dalton to pay, but the opinion has also been put forward that he was not sure how far professional jealousy would hinder his chances of becoming a member of that august assembly. This opinion is based upon the fact that Davy, who was at that time secretary of the Society and a most influential member of the Council, was by no means reconciled to Dalton's views on the atomic theory, and Dalton may have thought that this would prevent his being elected.

In 1816 he received the first mark of distinction when he was made a corresponding member of the French Academy of Sciences. He was always proud of this honour and there is an indication of just pride in the announcement of the fact of his election to his brother. He says in a letter dated October 30th, 1817:—

"I do not know whether I mentioned the fact that the French Academy of Sciences lately elected me a corresponding member on the subject of Chemistry, an honour that has been conferred only on one other person

in this kingdom, I believe, namely, on Dr. Wollaston, Secretary to the Royal Society. I received the diploma a few months ago."

In 1822 Dalton became a Fellow of the Royal Society. He was proposed, without his knowledge, and duly elected.

It was not until about 1815 that he came to be known as a man of mark in the scientific world, or rather it was not until then that his merits were fully appreciated by those who were in power at that time. One of the first marks of this appreciation was the offer of the post of scientific expert to the expedition to the Polar regions which the Government was about to despatch. The following letters describe the offer and its refusal :—

"23 Grosvenor Street, Feb. 12th, 1818.

"My Dear Sir,—You have probably heard of the Expedition which is preparing for investigating the Polar regions. The Royal Society charged by the Admiralty with the scientific arrangements of this voyage, is very desirous of making the most of so interesting an opportunity of investigating many important objects relating to meteorology and the theory of the earth.

"They have obtained from the Admiralty the power of recommending a natural philosopher to go on this Expedition; and it has occurred to me, that if you find your engagements and your health such as to enable you to undertake the enterprise, no one will be so well qualified as yourself.

"Probably £400 or £500 a year will be allowed during the voyage. It may last from one to two years; but most probably only three or four months: in all events, it is believed, that the whole year's salary will be allowed.

"This plan may not in any way fall in with your views. At all events, I am sure you will not be displeased with me for inquiring if such a proposition will be acceptable to you; and impute this letter to the high respect I have for your talents and acquirements, and the desire that they may be brought into activity, and estimated and rewarded as they ought to be.—I am, my dear Sir, very sincerely yours, H. Davy."

The letter in which Dalton replied to this offer states his reasons for declining, and is quite courteous, one might say more polished than his literary efforts usually were. It runs as follows:—

"Respected Friend,—In reply to your favour of the 12th instant, I may observe that the novelties of an enterprise similar to the intended one, can hardly fail to present much gratification to individual curiosity, and many interesting opportunities for observation of natural phenomena, particularly of meteorology, which certainly operate as strong inducements for me to engage in it; but on the other hand to quit the regular habits of a sedentary life for a sea-faring one, and that on a voyage of uncertain duration, and of more than ordinary risk, with the prospect of a cold, or at least, a desultory climate; add to these the interruption of my chemical

investigations which I have not yet been able to close, —these together outweigh with me any inducements which the proposed scheme can offer. I must therefore decline the acceptance of the proposition.

"At the same time, I would have it to be understood that I feel gratified by the communication of your favourable opinion of my scientific labours; and it will afford me great pleasure if I can in any other way contribute to the successful issue of the expedition, especially as far as it may be connected with the promotion of science.—I remain, with high esteem, your obliged Friend, JOHN DALTON.

"SIR HUMPHRY DAVY,
"23 GROSVENOR STREET, LONDON."

An important event in Dalton's life took place in 1822 when he made a visit to Paris. It was customary at this time for men of science to pay visits to the French capital in order to meet many of the distinguished scientists who lived and worked there, as well as for the purpose of seeing the libraries and museums. Added to which attractions there were the additional ones offered by this city of gaiety and culture. Dalton was not slow to avail himself of these many advantages, and Miss Johns, speaking of this visit, says:—

"His visit to Paris in 1822 perhaps afforded him as much, if not more pleasure, than any circumstance in his life.

"In that city he was received with a distinction, and met with those attentions which at that time had not been accorded to him in his own country,

and he ever retained a most grateful and affectionate remembrance of those who had shown a sense of his merit."

In Paris, he was received with the greatest hospitality and friendliness by his fellow-workers in chemistry. The society was of a kind which he could appreciate, for although entirely different from that to which he had long been accustomed, it included men of science of such world-wide fame as Cuvier and Laplace, Humboldt and Milne-Edwards, Gay-Lussac and Berthollet, besides others as great, such as Fourier, Ampère, Thénard and Cuvier. Nor did he confine his attentions solely to scientific matters, for he went to both the Italian Opera and the Théatre Français, and evidently saw a fair amount of the social life in Paris under the care of Mlle. Clémentine Cuvier.

It is to be regretted that Dalton's notes on his impressions of Paris and the records of his doings are brief, but it is worth quoting the scanty journal he did write, and further the remarks of one of his companions, a Mr. Dockray.

Dalton's first acquaintance in Paris was M. Brègnet, the mechanician and member of the Institute, upon whom Dalton called in order to have repaired a watch, originally made by Brègnet. On discovering the name of his client, M. Brègnet gave him the warmest of welcomes, and M. Brègnet *fils* wrote to Dalton the following letter :—

"Mon père a vu M. de la Place ce matin au Bureau des Longitudes. Il désire beaucoup vous voir et vous engage pour dimanche prochain à aller avec vos deux amis, diner chez nous à 5 heures precises; car nous

comptous toujours sur votre promesse de faire ce que vous pourrez pour cella."

At this dinner party he met MM. Arago and Fresnel and other well-known men. The further account of his stay in Paris and the excellent reception accorded him can be told in his own words.

"Saturday, July 6th, received a visit from two Swedish chemists from Abo, in Finland, pupils of Berzelius, Bornsdorf, and Nordenskiold. Visited the venerable Abbé Gregoire. 7th Sunday, Attended the service at the British Ambassador's chapel. From one to two hundred present, chiefly English, and more than half ladies. Very genteel and attentive congregation, good sermon well calculated for Paris, on the evidences of Christianity. After 4 p.m. took coach with companions for Arcueil, to dine by invitation with the Marquis Laplace and lady. Met Berthollet, Biot and lady, Fourier, etc., etc. A most agreeable and interesting visit, and a beautiful place. Monday, 8th July. Walked down to the arsenal; saw Gay-Lussac for half an hour; went to the Jardin du Roi, saw the wild beasts and the anatomical preparations, etc.; took coach home and then went to the Institute. About one hundred persons present. Was introduced by Biot and placed in the square adjacent to the officers; was announced by Gay-Lussac (as president) as a corresponding member (English) present. The sitting was from three to five o'clock. After my announcement my two companions were introduced to the same bench during the sitting. Sunday, 14th. Gay-Lussac and Humboldt called and spent an hour on meteorology, etc.

Took a coach to Thénard's; breakfast *à la fourchette* with him, family, and Dr. Milne-Edwards. Went to the laboratory near M. Biot's and saw a full set of experiments on the deutoxide of hydrogen, most curious and satisfactory. M. Thénard then went with us through the laboratory, showed us the new theatres for chemistry, physique, etc., and then went to M. Ampère's, who had previously prepared his apparatus for showing the new electro-magnetic phenomena. Saw a set of these experiments which, with the aid of Dr. Edwards, were made intelligible to me. 15th. Took coach to the Arsenal; spent an hour with Gay-Lussac in his laboratory; saw his apparatus for specific gravity of steam, vapours, etc., also M. Welters, the improver of chemical distillation, etc. Walked to the Jardin du Roi, *déjeuner à la fourchette* with Monsieur and Madame Cuvier and youngest daughter. M. Cuvier went with us to the museum, and accompanied us for some time, and then left a gentleman to attend us through the museum, being himself engaged, but occasionally meeting us; spent two hours in the museum—the most splendid exhibition of the kind in the universe—it beggars description. Left after two, and took a coach to the Institute; took a cup of coffee, etc., and then entered the library; saw and spoke MM. Milne-Edwards, Biot, Cuvier, Laplace, Berthollet, Brègnet, etc.; entered the Institute, heard papers by Milne-Edwards, Biot (on 'The Zodiac de Denderah'), Fourier (on 'The Population of Paris'), after which notice was given for strangers to withdraw, when Gay-Lussac called to me to stay, if I chose, being a member, which

I did. The business was about the election of members, and lasted nearly half an hour, after which we broke up. Saw M. Pelletan on coming out, who kindly inquired of me my health, etc.; went with Vauquelin in a coach to dine, when my companions met me; saw M. Payen, a young chemist of promise."

Dr. Lonsdale in his life of Dalton, in speaking of the first meeting of the Institute at which Dalton was present:—

"Dr. Robert Knox was present at the *séance*, and told me that on Mr. Dalton's name being announced, the president (Gay-Lussac) and the other members of the Institute rose from their seats, and bowed to the Manchester philosopher. Such honours, it was remarked at the time, were not offered to Napoleon le Grand when he took his seat amid the renowned Forty of France. I remember in the year 1838 seeing Lord Brougham enter the Institute without, however, eliciting any special mark of attention from its members."

The two companions whom Dalton mentions in his diary were Mr. Benjamin Dockray and Mr. W. D. Crewdson. Dockray gave Dr. Henry the following account of his recollections of this visit to Paris:—

"He (Dalton) was so entirely devoted to the interests of science, that, except in the intervals of relaxation, when he entered thoroughly into whatever interested those around him, his habitual manner, his caste of character, had acquired what may be considered a congenial equability that seemed to exempt him in great measure from personal influences, as well as from all undue susceptibility to considerations regarding his own individual repute or interest. I entirely believe, that

the original and memorable experiments, which Thénard and Ampère severally exhibited before him, gave him as much pleasure, since they were instances of success in eliciting the great truths of nature, and intimated still onward views to be afterwards pursued, as if they had been the result of his own researches. All his friends well knew that, notwithstanding his eminent position and the recondite nature of his studies, no one could more readily find interest or amusement in common things or incidents; and that no one's attention could more readily be engaged by the sort of novelties which occur in foreign travel. I was particularly struck by observing the impression made on Dr. Dalton by the solemnities of Roman Catholic worship, and the evident sincerity of profound devotion which he saw there; and I do not doubt that it was to him a page of human nature, which, till then, he had never had an equal opportunity of witnessing. Second, I think, only to this, for impressiveness of novelty, was the gallery of the Louvre. I do not doubt, but that he felt there was, in the masterpieces of art which he saw there, a new world of interest and wonder, on which he would gladly have had the opportunity of meditating longer."

The following account from the pen of the same writer is worth recording as it describes a dinner-party at which Dalton met some most distinguished men of science.

"*July* 7, 1822.

"At four in the afternoon by a coach with Dalton to Arcueil, Laplace's country-seat, to dine. Engaged the

carriage to wait for our return at nine. On alighting, we were conducted through a suite of rooms, where, in succession, dinner, dessert, and coffee tables were set out;—and onwards through a large hall, upon a terrace, commanding an extent of gardens and pleasure-grounds. There was a sheet of water in front, and a broad spreading current pouring into it from some rocks, where was seen a sculptured figure—an antique—found in the locality, representing the genius of the place. It is in these grounds that are still remaining the principal Roman works near Paris,—the vestiges of Julian's residence, as governor of Gaul. Avenues, parterres and lawns, terraces and broad gravel walks, in long vistas of distance, are bounded by woods and by higher grounds. As yet we had seen no one, when part of the company came in view at a distance: a gentleman of advanced years, and two young men. Was it possible not to think of the groves of the Academy, and the borders of the Ilyssus? We approached this group, when the elderly gentleman took off his hat, and advanced to give his hand to Dalton. It was Berthollet! The two younger were Laplace's son, and the astronomer royal—Arago. Climbing some steps upon a long avenue, we saw, at a distance, Laplace walking uncovered with Madame Biot on his arm; and Biot, Fourier, and Courtois, father of the Marchioness Laplace. At the front of the house, this lady and her grand-daughter met us. At dinner, Dalton on the right hand of Madame Laplace, and Berthollet on her left, etc. Conversation on the zodiac of Denderah and Egypt, Berthollet and Fourier

having been in Egypt with Napoleon; the different eras of Egyptian sculpture; the fact that so little at Rome of public buildings is earlier than Augustus, etc.

"After dinner, again abroad in the beautiful grounds, and along the reservoir and aqueduct of Julian. These ancient works, after falling very much into decay, were restored by Mary of Medicis. Dalton, walking with Laplace on one side, and Berthollet on the other, I shall never forget. . . . Laplace is an uncommon union of simplicity and manners and an essential dignity of character. His collected and serene air realizes to the observer the tranquillizing influence of philosophy."

During Dalton's stay in Paris, Dr. Milne-Edwards acted as interpreter between him and those scientists who did not speak English, and this was the beginning of an acquaintance continued by means of correspondence. One letter of Dalton's to Dr. Milne-Edwards is recorded in which he introduces an Englishman and his family. He goes on to say:—

"Permit me to return you my best thanks for your valuable essays *De l'Influence des Agents Physiques sur la Vie.* I have not had time to peruse all of them with that attention which they merit; but have seen enough to satisfy me that the subjects are treated with great skill and infinite labour in the experimental department. I regret that I have no recently printed essay to transmit to you. I have been working lately on the combustion of different gases. The heat seems nearly as the oxygen; but the light is subject to various modifications, which I have not yet completely ascertained."

As has been already stated, this visit to Paris was quite an important event in the life of the quiet philosopher. Miss Johns records in her diary that on his return he showed an unusual amount of vivacity and communicativeness, so much so that he was frequently accused of having become "half a Frenchman." On the whole, however, this sober Quaker did not find the customs of the gay French people altogether to his liking. Paris must have seemed strange and unnatural to one of his reserved nature. Probably, in that gay society he was regarded as somewhat strange; for though English people are always regarded as reserved, he surely possessed this character to a greater extent than is usual. At least one story has been preserved which illustrates the opinions of his French scientific contemporaries, and their view of his character. One evening in Manchester some distinguished members of the French Academy called on purpose to see the celebrated chemist. As usual amongst strangers he was silent and reserved, and scarcely exchanged a word with his visitors. As soon as tea was over, he called for his lantern and went away to his laboratory without one word to his guests. One of these latter, on being asked by Mr. Johns what he thought of the philosopher, replied, "Monsieur Dalton a une simplicité admirable."

One further episode in Dalton's visit to the French capital is worthy of mention. He was introduced to Mlle. Clémentine Cuvier, daughter of the distinguished Baron Cuvier, and seems to have been much charmed by her. He delighted in being in her company and visited many places of interest in Paris under her

direction. He thought her, says Miss Johns, "the most attractive and amiable young creature that he had ever seen," and greatly lamented her early death. Speaking of her on one occasion to a friend, he said, "Ah! she was a bonny lass; she treated me like a daughter."

In 1825 George IV. founded two annual prizes of the value of fifty guineas each, to be awarded by the President and Council of the Royal Society, and in 1826 the first Royal Medal was awarded to Dalton. Sir Humphry Davy announced the award as having been made to "Mr. John Dalton, for the development of the chemical theory of Definite Proportions, usually called the Atomic Theory, and for his various other labours and discoveries in physical and chemical science."

Sir Humphry Davy as President, presented the medal to Dalton and at the same time stated Dalton's claims to the honour. His appreciation on behalf of the Society he conveyed in the following words:—

"To Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject. Finding that in certain compounds of gaseous bodies, the same elements always combined in the same proportion; and that when there was more than one combination, the quantity of the elements always had a constant relation, such as 1 to 2 or 1 to 3, or to 4, he explained this fact on the Newtonian doctrine of indivisible atoms, and contended, that the relative weight of one atom to that of any other atom being known, its proportions or weight in all its combinations, might be ascertained; thus making the statics

of chemistry depend upon simple questions, in subtraction or multiplication, and enabling the student to deduce an immense number of facts, from a few well-authenticated, accurate, experimental results. Mr. Dalton's permanent reputation will rest upon his having discovered a simple principle, universally applicable to the facts of chemistry—in fixing the proportions in which bodies combine, and thus laying the foundation for future labours, respecting the sublime and transcendental parts of the science of corpuscular motion. His merits in this respect resemble those of Kepler in astronomy. . . . Mr. Dalton has been labouring for more than a quarter of a century, with the most disinterested views. With the greatest modesty and simplicity of character he has remained in the obscurity of the country, neither asking for approbation, nor offering himself as an object of applause. He is but lately become a fellow of this Society, and the only communication he has given to you, is one, compared with his other works, of comparatively small interest; the feeling of the Council on the subject is therefore pure. I am sure he will be gratified by this mark of your approbation of his long and painful labours. It will give a lustre to his character which it fully deserves; it will anticipate that opinion which posterity must form of his discoveries; and it may make his example more exciting to others, in their search after useful knowledge and true glory."

From this time onwards, honours were showered upon him. In 1830 he was made one of the eight foreign associates of the French Academy of Sciences in

the place of Davy. This is perhaps the highest honour that can be paid to a man of science, nor was Dalton unaware of the fact. He valued these marks of appreciation of his position in the scientific world just as he valued the friendship of men similarly distinguished, men like Dr. Chalmers, Sir David Brewster, and Professor Thomson. Universities at home and abroad were delighted to add his name to their rolls of men famous in science and letters. In 1832, on the occasion of the second meeting of the British Association for the Advancement of Science at Oxford, the University conferred upon him the degree of D.C.L. and bestowed the same honour upon Brewster and Faraday. During his stay in Oxford he had rooms in Queen's College and seemed to take much delight in walking through the gardens wearing his scarlet robes. As a Quaker, he should have avoided such brilliant colours, but was able to satisfy his conscience on the ground that in his eyes, the gown was of the colour of the green leaves around him.

Learned societies were proud to number him amongst their members. Berlin, Moscow, Munich, paid tribute to his work, Edinburgh made him a Doctor of Laws, and all these honours, though he valued them at their proper worth, left him the same simple unaffected man that he had always been. Knowing his own powers, he yet remained modest in his demeanour and simple in his tastes. As Davy said of him, he neither asked for approbation nor offered himself for public applause. He possessed, as his French critic said, a most delightful simplicity of character. Honours came to him by right,

but they were not of his seeking. His country, somewhat tardily, rewarded his life's work in the service of science by giving him a pension, but for this reward he did not strive; it came to him as the result of the labours of his friends who were both faithful and persistent. The story of how the pension came to be given and the dramatic manner of the announcement are interesting, and will be given in the next chapter.

CHAPTER XII

GOVERNMENT PENSION TO DALTON

In 1829, Mr. Babbage, whose name is chiefly known in connection with the calculating machine, wrote to Dr. Henry (the father of Dalton's biographer), suggesting that the Government should show its appreciation of Dalton's services in the interest of science by awarding him a pension. The history of the efforts made to secure this reward may be read in the following letters and extracts.

In a letter to J. A. Murray, Esq., M.P., dated 26th February 1833, Dr. Henry writes:—

"About four years ago an inquiry was put to me by a friend (Mr. Babbage) which I judged to be the forerunner of some act of Government in favour of Dr. Dalton, the venerable President of our Literary and Philosophical Society. Since that time several letters have passed between Mr. Babbage and myself on the subject; but, notwithstanding his zeal in favour of Dr. Dalton's claims, nothing has yet been done. Under these circumstances, it appears to me exceedingly desirable, by the vigorous and united efforts of all who approve the measure, to anticipate the time, which cannot be far distant, when this illustrious man (now I believe in his sixty-sixth or sixty-seventh year) will be beyond the reach of sublunary reward. Our county

member, Mr. G. W. Wood, has kindly agreed to devote his best efforts to furthering the cause. We fully appreciate the difficulty of obtaining the grant of a pension (now a word of fearful omen especially to the grantors), but we are yet encouraged to hope for success by the soundness of Dr. Dalton's claims, and the little probability, from his rare excellence as a philosopher, that demands of equal weight can be made in favour of any other individual."

The following statement of Dalton's case was prepared by Henry and was submitted together with a formal application by Babbage, to Lord Grey, Lord Brougham, Mr. Poulett Thomson and others:—

"Mr. Dalton never had, nor was ever given to expect, any reward or encouragement whatsoever from government;[1] and having been in habits of unreserved communication with him for over thirty years, I can safely aver that it never occurred to him to seek it. He has looked for his reward to purer and nobler sources,—to a love of science for its own sake; to the tranquil enjoyments derived from the exercise of his own faculties, in the way most congenial to his tastes and habits, and to the occasional gleams of more lively pleasure, which have broken in upon his mind, when led to the discovery of new facts, or the deduction of important general laws. By the moderation of his wants, and the habitual control over his desires, he has been preserved from worldly disappointments; and by the calmness of his temper, and the liberality of his views,

[1] This clause was inserted to answer a question put by Mr. Babbage to Dr. Henry.

he has escaped those irritations that too often beset men who are over anxious for the possession of fame, and are impatient to grasp prematurely the benefits of its award. For a long series of years he bore neglect, and sometimes even contumely, with the dignity of a philosopher, who though free from anything like vanity or arrogance, yet knows his own strength; estimates correctly his own achievements, and leaves to the world, generally though sometimes slowly just, the final adjudication of his fame. Among the numerous honours that have since been conferred upon him, by the best judges of scientific merit in this and other countries, not one has been sought by him. They have been, without exception, spontaneous offerings, prompted by a warm and generous approbation of his philosophical labours, and by the desire to cheer him onward in the same prosperous career. Deeply as he has felt these distinctions, they have never carried him beyond that sober and well-regulated love of reputation, which exists in the purest minds, and is one of the noblest principles of action.

"In perfect consistency with Mr. Dalton's intellectual qualities, are the moral features of his character; the disinterestedness, the independence, the truthfulness, and the integrity which, through life, have uniformly marked his conduct towards others. He has been taxed with plagiarism, but never was a charge more completely unfounded. Not only is he incapable of encroaching upon the just rights of others, but even of taking *tacitly* to himself applause to which he does not feel that he is fully entitled. Of the work, from which he is accused of having borrowed the outline of the

atomic theory, he had never even heard, until many years after the publication of his views on that subject. Nor is this at all extraordinary, when it is considered that men, like Mr. Dalton, of original and creative minds, trust rather to their own powers of research, than to reading; and, in the knowledge of the history of science, are often surpassed by very inferior persons. This general remark applies to Mr. Dalton: but he is a *discoverer* in the true sense of the word. He has drawn from observed phenomena, new and ingenious views; upon these views he has founded distinct conceptions of a general law of nature; and he has traced out the conformity of that law with an extensive class of facts, many of which he himself revealed by well-devised experiments. He has thus secured our admiration, not by having broached ingenious opinions merely, but by having worked out the evidences of these opinions by labour most sagaciously and perseveringly applied. Nor is it on the atomic theory only that his reputation must rest. It has a broader basis in his beautiful and successful investigations into the subject of heat; into the relations of the air and moisture to each other, —and into a variety of other topics, intimately connected with the stability and advancement of chemical philosophy.

"I therefore agree with you, that Mr. Dalton has strong claims upon the national respect and gratitude, and contend for his title to reward, even though he may not have accomplished anything bearing *directly* upon the improvement of those arts and manufactures, which are the chief sources of our national wealth. For let it be

remembered that every new truth in *science* has a natural and necessary tendency to furnish (if not immediately, yet at some future time) valuable rules in *art*. Nothing is more common than that a general principle, when first developed, may admit of no obvious practical use; but that a few subsequent discoveries, made perhaps at a small expense of genius or labour, supply links, which render it available, first to individual, and in due course to public wealth and prosperity. Not to mention other instances, Mr. Watt derived from Dr. Black's discovery of latent heat a guiding light to the noblest invention that has ever been placed in the hands of man, for giving him control over the physical world, and even for advancing his progress in moral and intellectual cultivation. The discovery of chlorine also, in the laboratory of a retired chemist, brought forth no practical benefits for several years; but when found by a subsequent philosopher, to quicken the whitening of unbleached cotton and linen goods, it was immediately applied by practical men to the art of bleaching; and no one can now calculate its immense influence in giving rapid circulation to the capital employed in the cotton and linen manufactures. Among the abstract truths unfolded by Mr. Dalton, it would not be difficult to point out the germs of future improvements in the practical arts—germs which now lie dormant in the shape of purely scientific propositions.

"But were it otherwise, it would surely be unworthy of a great nation, to be governed in rewarding or encouraging genius, by the narrow principle of a strict barter of advantages. With respect to great poets and

great historians no such parsimony has ever been exercised. They have been rewarded (and justly) for the contributions which they have cast into the treasury of our *purely intellectual* wealth. And do not justice and policy equally demand that a philosopher of the very highest rank, one who has limited his worldly views to little more than the supply of his natural wants, and has devoted for more than forty years the energies of his powerful mind to enlarging the dominions of science, should be cherished and honoured by that country, which receives by reflection the lustre of his well-earned fame? The most rigid advocate of retrenchment and economy cannot surely object to the moderate provision, which shall exempt such a man, in his old age, from the irksome drudgery of elementary teaching, and shall give him leisure to devote his yet vigorous faculties to reviewing, correcting, applying and extending, what he has already, in great part, accomplished. In one instance of recent date, a philosopher who has eminently distinguished himself in purely abstract science, has received the merited reward of a pension for life. It is most desirable then that the British Government, by extending its justice to another not less illustrious, should be spared the deep reproach which otherwise assuredly awaits it, of having treated with coldness and neglect, one who has contributed so much to raise his country high among intellectual nations, and to exalt the philosophical glory of his age."

Considerable difficulty was experienced in obtaining the pension for Dalton for, as Mr. J. A. Murray wrote, at the time there was a "dislike to pensions approaching

to fanaticism." He says, "It is the cry of the times, arising no doubt from former abuses, and while it subsists to such a degree it is hardly possible to obtain for the most deserving what the nation ought to feel a pride in bestowing."

This admirable statement by Dr. Henry was, however, submitted by Mr. Murray to the Lord Chancellor, who admitted the justice of the application and expressed himself as being most anxious to make provision for Dalton, but found that the matter was attended with great difficulty.

The difficulties proved so great that it was not until 1833 that the reward was bestowed upon Dalton, and these difficulties would, in all probability, never have been surmounted but for the exertions of Mr. G. W. Wood and Mr. Poulett Thomson. Dalton was not informed of his success until the meeting of the British Association, which was held that year in Cambridge. Professor Sedgwick, the great geologist, was President, and to him the announcement was made immediately before the opening meeting.

In the Senate House of the ancient University, surrounded by all the greatest men of science of the nation, Dalton heard of his good fortune from the lips of Professor Sedgwick who, in eloquent language paid his tribute of praise to the great chemist. In the course of his presidential address he expressed himself thus:—

"They had all read a highly poetical passage of a sacred prophet, expressed in language, to the beauty of which he had never before been so forcibly awakened

as at that moment:—'How beautiful upon the mountains are the feet of him that bringeth good tidings.' If he might dare to make an adaptation of words so sacred, he would say, that he felt himself in the position here contemplated of one who had the delightful privilege of announcing good tidings, for it was his happiness to proclaim to them what would rejoice the heart of every true lover of science. There was a philosopher sitting among them, whose hair was blanched by time, whose features had some of the lines of approaching old age, but possessing an intellect still in its healthiest vigour—a man whose whole life had been devoted to the cause of truth; he meant his valuable friend, Dr. Dalton. Without any powerful apparatus for making philosophical experiments—with an apparatus, indeed, many of them might almost think contemptible—and with very limited external means for employing his great natural powers, he had gone straightforward in his distinguished course, and obtained for himself in those branches of knowledge which he had cultivated, a name not perhaps equalled by that of any other living philosopher in the world. From the hour he came from his mother's womb, the God of Nature had laid His hand upon his head, and had ordained him for the ministration of high philosophy. But his natural talents, great as they were, and his almost intuitive skill in tracing the relations of material phenomena, would have been of comparatively little value to himself and to society, had there not been superadded to them a beautiful moral simplicity and singleness of heart, which made him go on steadily in

the way he saw before him, without turning to the right hand or to the left, and taught him to do homage to no authority before that of truth. Fixing his eye on the highest views of science, his experiments had never an insulated character, but were always made as contributions towards some important end—were among the steps towards some lofty generalisation. And with a most happy prescience of the points towards which the rays of scattered experiments were converging, he had more than once seen light, while to other eyes all was yet in darkness—out of seeming confusion had elicited order, and had thus reached the high distinction of becoming one of the greatest legislators of chemical science.

"While travelling among the highest mountains of Cumberland, and scarifying the face of Nature with his hammer, he (the President) had first the happiness of being admitted to the friendship of this great and good man, who was at that time employed, day by day, in soaring among the heavens, and bringing the turbulent elements themselves under his intellectual domination. He would not have dwelt so long on these topics, had it not been his delightful privilege to announce for the first time (on the authority of a Minister of the Crown who sat near him [1]) that His Majesty King William the Fourth, wishing to manifest his attachment to science, and his regard for a character like that of Dalton, had graciously conferred on him, out of the funds of the Civil List, a substantial mark of his royal favour."

In no more appropriate way nor from a more fitting

[1] Lord Monteagle.

source could Dalton have learned the high mark of public appreciation. There can be now no question of the justice of the reward for many years of service in the interests of science. That a man of such high repute in the intellectual world, who had given so much of his time and energy to the elucidation of the mysteries of natural phenomena, should be compelled to earn his daily bread by the hard and somewhat thankless work of elementary teaching, was a condition of things that could not well be tolerated. It must have caused considerable delight to one who had never sought worldly advantages, to receive the announcement of public gratitude and appreciation from the lips of so celebrated a man as Adam Sedgwick, in a place that was historic, and surrounded by his peers in the scientific world, men whose names have been handed down to posterity with his own as earnest seekers after truth.

Gratifying, too, must have been the official announcement which was made by Poulett Thomson in the following letter :—

"Whitehall, *June* 22, 1833.

"SIR,—Although I have not the honour of enjoying your personal acquaintance, the gratitude which I feel for the distinguished services you have conferred upon science, as well as the respect which I entertain for your character, made me feel deeply anxious that some public mark of those feelings which are, I believe, common to the country, should be shown by the Government to which I belong, and induced me earnestly to press for the first opportunity being taken

to offer such a testimony, however trifling the pecuniary amount of it might be. It is therefore with sincere pleasure that I inform you that His Majesty has been graciously pleased to second their wishes in the manner, which you will perceive from the accompanying note from Colonel Grey, which I have the honour to enclose.

"I beg to subscribe myself, with the most sincere respect, Sir,

"Your faithful servant,

"C. POULETT THOMSON."

The note mentioned by Thomson and forwarded by him to Dalton was as follows :—

"DEAR THOMSON,—My father desires me to tell you that the King has been pleased to grant a pension of £150 nett to Mr. Dalton.

"Yours very truly,

"C. GREY."

Three years later, in 1836, Thomson again wrote to Dalton to inform him that he had represented to Lord Melbourne the desirability of increasing the amount of the pension, and stated that he was glad to be able to report that his Lordship had "given directions that a pension of £150 should be added to that already held."

At the same time when efforts were being made to secure the recognition, by the country, of Dalton's services, his friends in Manchester were endeavouring to establish a fund for an expression of their appre-tion of his character and work. It was proposed to

devote the funds either to the building and endowing of an institution for scientific research, or to the erection of a statue. In a letter to Mr. Ewart, Dr. W. Henry states the case in the following terms :—

"I trust that the Committee will pause before they decide to abandon the proposition of a statue, and will decide on handing down to distant posterity, the *viva ac vera effigies* of a man who will be honoured in all future ages, so long as science shall be known and respected. It will be a bequest which future philosophers, as well as the world at large, will know how to appreciate. It will gratify the desire inherent in all men, to call up distinct conceptions of the visible forms and features, which have been associated with intellectual endowments of the highest order. How much such resemblances are prized will appear from the following extract of a letter recently addressed by Berzelius (one of the first of living philosophers) to a friend (Dr. Traill), who had sent him the portrait of Dr. Dalton.

"'Mille et mille remercîments pour ce cadeau. Je suis bien aise d'avoir une idée de la figure d'un homme, dont une pensée heureuse a été si fertile en résultats scientifiques.'"

Eventually the supporters of the scheme in favour of the statue triumphed, and Dr. Henry, on behalf of the Committee, made the following report :—

"*February* 28, 1834.

"The Committee appointed to take measures for obtaining a statue of Dr. Dalton have great pleasure in reporting that the object is now in a fair way to be

accomplished. Finding the proposal to be warmly seconded by the general view of the inhabitants of Manchester and the surrounding districts, and to be supported by willing and liberal contributors, it appeared to them to be time to seek such information as might enable them to decide to what sculptor a work destined, it is hoped, to last for ages, should be confided. With the highest respect for other artists, whose names have been mentioned, some of them established in celebrity, and others fast rising into reputation, the Committee have fixed their view on Mr. Chantrey, as one eminently distinguished, not only in the inventive province of his art, but by the fidelity with which, in more than one recent instance, he has portrayed the lineaments of men grown old in intellectual pursuits, and by the success with which he has caught that expression of calm and patient thought, and that capacity for lofty contemplation, which nature and habit had imprinted on their features.

"To the inquiry addressed to Mr. Chantrey for information respecting his usual terms, that gentleman has replied in a liberal spirit, not insisting on those terms to their full extent, and declaring that the leading incitement with him to execute the work to the best of his ability, will be his heart-felt respect for the subject of the memorial, and his anxiety to be remembered by posterity in connection with so great a name.

"The Committee, by a deputation consisting of its chairman and a few other members, communicated to Dr. Dalton their wish that he should afford to Mr. Chantrey the necessary facilities, at the earliest period,

that will suit their mutual convenience; and in this request he acquiesced with the modesty, simplicity, and excellent feeling that grace his character. There is, therefore, every reason to hope, that at a period not much exceeding two years, we shall possess and hand down to posterity, protected from decay or injury within the walls of the Royal Institution, into which the governors have cordially agreed to receive it, a noble work of art, not only a memorial of our high respect for Dr. Dalton, and of the pride we take in him as a fellow-townsman, but a proof that *we* in *our* generation were not incapable of estimating the genius of the philosopher, nor slow in paying homage to the virtues of the sage."

In May 1834 Dalton, accompanied by his biographer Dr. Henry, went to London to meet the sculptor. The sittings seem to have been characterized by good-fellowship between the philosopher and the sculptor, and an account of them is given in the following letter, dated 2nd May, 1834, to Peter Clare:—

"Next morning Mrs Wood[1] walked through the park with me to Mr. Chantrey's, when we found him in expectation of seeing me. He took a profile as large as life by a camera lucida, and then sketched a front view of the face on paper. We took a walk through his rooms, and saw busts and statues without end. He then gave me the next day for a holiday, and told me I should see my head modelled in clay on Wednesday morning, at which time he invited me to breakfast. I went accordingly, and found, as he said,

[1] The wife of Mr. G. W. Wood, M.P. for S.W. Lancashire.

a head *apparently* perfect. He said he had not yet touched it, the head having been formed from his drawings by some of his assistants. He set to work to model and polish a little whilst I was mostly engaged in reading the newspaper, or conversing with him. On looking right and left he found my ears were not alike, and the modeller had made them alike, so that he immediately cut off the left ear of the bust, and made a new one more resembling the original. Most of the time I was amusing myself with viewing the pictures and statues in the room. At last he took a pitcher and blew a little water in my face (I mean the model), and covered my head with a wet cloth and we parted, he having desired me to bring Dr. Henry and Dr. Philp with me next morning to breakfast. We went accordingly, and found an abundant table; soon after Dr. Faraday came in, and we all went into the working room for a time. This morning (sixth day) Mrs. Wood was kind enough to walk with me again to Mr. Chantrey's, and we spent another hour or two under his directions. At intervals we have a little amusement and instruction about our respective arts and sciences, and how we acquired our knowledge, etc., in which we vie with each other and keep up a lively conversation."

From the bust, a statue was afterwards modelled, and this stands in the main entrance of the Manchester Town Hall. Dr. Henry says: "A more refined and ideal expression has been bestowed upon the countenance in the statue. The bust is the more faithful portraiture of the philosopher." In addition to the statue in the

Town Hall, Manchester possesses a bronze replica which is placed in the Infirmary Square, together with statues of Peel, James Watt, and Wellington, and the town in which Dalton spent so many years of his life and in which he did his life's work has further perpetuated his memory by the establishment of scholarships. On 26th January 1853, a meeting of the townspeople of Manchester was held in order to discuss a scheme for the foundation of scholarships for the furtherance of original research in chemistry. This scholarship was to be held in the Owens College which had just been established, and in connection with this scheme no apology is needed for the quotation of Sir Henry Roscoe's remarks on the subject. He says:—

"A sum of £4000 was raised by public subscription, and a more fitting testimonial could not have been proposed. The establishment of a scholarship for scientific research was at that time a circumstance without a parallel; but in spite of the novelty of the experiment, the experience of forty years[1] has fully borne out the wisdom of the course which its originators adopted, and to-day a long list exists of 'Dalton Scholars' who have contributed to the progress of chemical science, and many of whom hold high and responsible positions in scientific, manufacturing and official life."

The last few years of Dalton's life differed in no very marked manner from the earlier ones. In 1830 the Johns family left Manchester, and Dalton took a house close to where he had lived so many years, and

[1] These words were written in 1895.

there lived alone until his death in 1844. From 1837 onwards his health was feeble, and he had several paralytic seizures which affected both mind and body, though until the last he continued to record his meteorological observations.

It is quite easy to see that, during the last few years of his life, Dalton's mental and physical powers failed rapidly. He himself was quite conscious of the change for he wrote to Murchison saying, "I succeed in doing chemical experiments, taking about three or four times the usual time, and I am long in calculation." On another occasion, speaking of a French *savant* he said, "Aye, he was a nonentity, as I am now."

In 1834 his brother Jonathan died of paralysis. Like his more famous brother, Jonathan had an attack some years before the one which caused his death.

In the following letter to a distant relative, Dalton announced the death of his brother and gave some account of his financial affairs :—

"KENDAL, 14th, *December* 1834.

"RESPECTED FRIEND HENRY DALTON—It falls to my lot to have the melancholy task of informing thee of the decease of my brother Jonathan Dalton ; he died on the morning of the 11th inst. after an increased severity of his affliction for a few days ; I believe all the alleviations that could be availing were afforded ; I received the account on the evening of the 11th, and arrived here on the 12th. The funeral is fixed for to-morrow morning.

"I find by his will he has left all his real and personal

estate to me, and made me the sole executor; the real estate thou art acquainted with; the personal is very small, and the debts are considerable, amounting to nearly £900; but about one-half of this was owing to me.

"I find thy letter of the 9th July 1834, with a note that it was answered by him on the 12th. I understand that my brother wrote thee (or rather got an amanuensis to write) about three or four weeks since, and no answer having come to hand yet, I judge it expedient to inform thee of the present circumstances without delay.

"As I have no doubt thy agency has been satisfactory to my brother, I hope it will be continued to me: when the late half year's rent is received and disbursements paid I think it will be best to remit to W. D. Crewdson & Son, bankers here, to be placed to my account with them, and to request from them an acknowledgement of the receipt, and they may inform me as may be convenient.

"Should anything occur to require my attention, my address will be at 40 George Street or 27 Falkner Street, but 'Dr. Dalton, Manchester,' will generally find me.

"At my distance from Eaglesfield I cannot often visit it; but no one knows what may happen.

"During my stay of two or three weeks in London last spring, I had occasion to call once or twice on my namesake in Regent Street.

"I do not pay postage in order to secure a more careful delivery.

"With my respects to thyself and family, and to my few remaining friends at Eaglesfield, I remain, thine sincerely, JOHN DALTON.

"*P.S.*—I shall remain two or three days here."

Dalton's money affairs were now such as to make his last few years comfortable. He seems to have given up his teaching work and to have lived upon his pension and the income derived from the Eaglesfield estate.

His life was very uneventful; he continued to some extent his scientific work and attended various meetings of the British Association. In 1835 the meeting was in Dublin, in 1836 in Bristol, and on each occasion Dalton was present in his capacity as Vice-President of the Chemical Section, but communicated no paper at either meeting. In 1837, the Association met in Liverpool, but Dalton in consequence of a paralytic seizure was not present. The Earl of Burlington stated that "the venerable Dr. Dalton was absent; the infirmities of increasing age had compelled him to abstain from meeting those who delighted to honour the philosopher, whose life had been devoted to science, and whose reward had come late; but it was a reward whose justice all acknowledged, and the honours conferred on Dr. Dalton were as gratifying to the public as to himself."

It must have been a matter for regret both to Dalton and the scientific world, that when the British Association met in Manchester in 1842, Dalton was so unwell as to be unable to fill the post of President, for there

could have been no more suitable person to preside over the assembly of scientists in Manchester, than one who had lived so many years within its gates, and who had there carried out his researches and elaborated a theory which has gained for its author an undying fame.

Lord Francis Egerton was President and alluded to Dalton in the following words :—

"Manchester has, in my opinion, a claim of equal interest as the birthplace,[1] and still the residence and scene of the labours, of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive, if he will condescend to do so, from myself the expression of my own deep personal regret that increase of years, which to him up to this hour, has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which, in his case, would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting, in his native town, from being associated with the name of Dalton as its President. The Council well know my views and wishes in this matter, and that, could my services have been available, I would gladly have served as a doorkeeper in any house where the father of science in Manchester was enjoying his just pre-eminence."

As has been stated, Dalton was attacked by paralysis

[1] Because of Dalton's long residence in Manchester, many people had come to regard it as his birthplace.

in 1837, and suffered from several recurrences of the same disease. Mr. Ransome, who was his medical attendant during the later years of his life gives the following particulars of his illness:—

"On the 18th of April 1837 I received an urgent summons to Dr. Dalton before breakfast.

"I found him pale, speechless, and paralysed on the right side; he appeared to recognise me by attempting to speak, and moving his left hand towards me.

"It appeared that he had risen at his usual time, had dressed without assistance, and had entered in his meteorological journal the temperature, maximum and minimum, of the previous night; his hand had evidently been very unsteady, from the tremulous and indistinct figures. After this I believe that he fell, and was discovered in the state in which I found him on my arrival.

"On the previous evening, he had received a visit from a distinguished chemist, with whom he had a long, animated, and somewhat warm discussion on the subject of chemical notation and symbols; he contending that his own plan of representing the probable position of the atoms of a compound was preferable to the system then and now universally adopted, which merely gives the initials of the elements and the number of their atoms. After the departure of his visitor, he appeared more excited than usual, and unable to let the subject drop and to subside into his usual calm.

"A consultation with the late Dr. Holme and yourself[1] was immediately arranged, at which it was decided that

[1] Dr. W. C. Henry.

his condition, from great prostration, would not admit of any depletory or reducing measures.

"In a few hours he rallied somewhat, and in a few days recovered, in great measure, the use of the right side, but articulation and the memory of words remained impaired for a much longer time; indeed I may say they were never completely restored to their former normal condition.

"You may probably remember his impatience at the occasional doses of medicine which were strictly insisted upon by our senior colleague, and the amusing diary with hourly remarks, which were duly kept at his request by his kind and indefatigable friend, the late Mr. Peter Clare, in which were recorded with great minuteness the effects of medicines, his own sensations, etc.

"As nearly as I can recollect, about the end of May I accompanied him in his first drive out to the Botanical Gardens, which the Council had kindly placed at his disposal as a place where he could take exercise with great privacy during his convalescence.

"During our walk through the conservatories, he conversed freely though somewhat indistinctly with the gardeners, and requested that they would forward to him bottles of air from the greenhouses after prolonged closure.

"In about six weeks after this, he resumed his usual avocations and excepting for some slight ailment, as catarrh, I discontinued my *professional* attendance.

"In the following year, 1838, on the 15th of February, he had a second slight attack, from which he

soon rallied; after which he remained free from any decided attack, although his strength, particularly his physical, failed so decidedly as to require constant assistance, until May 20, 1844, when another slight fit occurred.

"A few weeks after this, he received a vote of thanks from the Literary and Philosophical Society for his valuable contributions to its Memoirs, to which he was unable to reply orally, but delivered a written acknowledgment.

"Lastly, on the 27th of July immediately following, I was again hastily summoned, but only to behold the lifeless body of my venerable friend."

The following description of Dalton's last hours is given by Dr. George Wilson:—

"On Friday, the 26th of July, he retired to his room about a quarter or twenty minutes after nine o'clock; and going to his desk, on which were usually placed the books in which he recorded his meteorological observations, he entered therein the state of the barometer, thermometer, etc., at nine o'clock, and added in the column for remarks, the words 'little rain,' denoting that but little had fallen during the day. His servant observed that his hand trembled more than he had ever before seen it, and that he could scarcely hold the pen. Indeed, the book exhibits, in its tremulous characters and blotted figures, striking proof of the rapid decay of the physical powers. But there was the same care and corrective watchfulness as ever, manifested in this the last stroke of the pen; for having written opposite a previous observation, 'little rain this,' he now noticed that the sentence was incomplete, and added the word

'day,' which was the last word that was traced by his tremulous pen. He retired to bed about half-past nine, and spent a restless and uneasy night, but seemed, on the whole, in his usual way when his servant left his bedside at six o'clock next morning.

"About half an hour later, his housekeeper found him in a state of insensibility, and before medical attendance could be procured, though it was immediately sent for, he expired, 'passing away without a struggle or a groan, and imperceptibly, as an infant sinks into sleep.'"

Dr. Wilson and Mr. Ransome made a *post-mortem* examination, and paid particular attention to the eyes. This was done, as Dalton had expressed the wish that his eyes should be examined in order to ascertain if there were any connection between the anatomy of the eye or the colour of the vitreous humour, and the abnormal colour vision. As we should expect in the light of present-day knowledge, no such connection could be discovered.

Such was the end of the great philosopher. He died as he had lived, quietly and simply, without any of the pomp and show which sometimes attend the departure of the great.

It was universally wished in Manchester to pay honour to the memory of one of its most distinguished sons, and accordingly, in spite of the protest of the Society of Friends, it was decided that the funeral should be a public one. The body was placed in a darkened room in the Town Hall and was inspected by more than forty thousand persons. On Monday, 12th August, the public funeral took place. A procession was formed

of nearly a hundred carriages, and many hundreds went on foot to the grave in Ardwick Cemetery. Police wearing emblems of mourning lined the route, shops and warehouses were closed as the funeral procession accompanying the remains of the philosopher passed to his last resting-place.

The grave is covered by a massive block of polished red granite, on which are inscribed his name, and the dates of his birth and death.

Dalton had accumulated a small fortune during the course of a long and careful life. He succeeded, at his brother's death, to a small family estate, and this together with about £8000 personal estate constituted the whole of his worldly goods of which he died possessed.

His will was made on 22nd December 1841, and in this he bequeathed £2000, "to found, endow, or support a Professorship of Chemistry at Oxford, for the advancement of that science by lectures, in which the Atomic Theory, as propounded by me, together with the subsequent discoveries and elucidations thereof, shall be introduced and explained." This bequest was, however, revoked by a codicil made in 1843, in which he made more adequate provision for the family of one of his oldest friends, the Rev. W. Johns, who had suffered pecuniary losses late in life. His real estate he bequeathed to Mr. Nield and Mr. Peter Clare. Of his personality, he left £500 to the Quakers' School at Ackworth, Yorkshire, £300 to a similar school at Brookfield, and £50 to the Eaglesfield and Blind Bothell School. The residue he divided amongst his friends and relatives.

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